THE LANCET.

VOL. I.] LONDON, SATURDAY, MARCH 7. [1828-9.

LECTURES

ON THE

DISEASES OF WOMEN AND CHILDREN.

DELIVERED AT GUY'S HOSPITAL BY

DR. BLUNDELL.

LECTURE XVIII.

Of the indolent Scirrhus of the Uterus.

IN the same manner as there is much variety in the features of the face, so also there is much variety in the make of the os uteri in different individuals. In some women the os uteri is very small, firm, and flat; in others, again, very large in its size, and soft and projecting: sometimes, in those who have borne children especially, the aperture which leads into the uterus is so capacious, that one or two fingers may pass it; and sometimes it is so small, that you can scarcely ascertain its existence, except by carrying up some instrument, such as a blunted probe, for example, and which may generally be slipped into the uterine cavity: in the general, the surface of the os uteri is tolerably smooth, but not always, for a sort of inequality may be felt, sometimes arising from rugosity of the inner membrane investing these parts, and sometimes, perhaps, from lacerations of the uterine substance, which have occurred during parturition.

Of the os uteri, there are three forms which are most frequently observed in practice, the one the flat os uteri, the other the os uteri formed into a rounded tubercle, and penetrated by a circular aperture; the third the os uteri projecting like a tubercle into the vagina, and divided by a transverse fissure, (for the fissure never stretches from before backward,) so as to become separated into an anterior and posterior lip.

To illustrate these points, I may now circulate the preparations before me; and you have here a specimen of the tubercular os uteri, in form opposed to that flattened os uteri to which I before referred.

In the preparation which follows, the flat os uteri is well demonstrated, the mouth, in both cases, being perforated by circular apertures.

Here is a third preparation, exhibiting the transverse fissure with the labella, the one anteriorly, the other behind.

Next follows a specimen of the os uteri in a state of rugosity, free, however, from the changes of serious disease.

In the preparations which follow, may be seen specimens of the large os uteri and the small, with the capacious and with the contracted aperture, the last being more especially deserving of attention, because, the aperture being overlooked altogether, the oversight may give rise to strange mistakes respecting the nature of disease.

Scirrhus.

Among the various diseases to which the womb is obnoxious, not the least important is the indolent scirrhus; and of those cases of scirrhosity there are three varieties, the knowledge of which is of practical importance to the accoucheur. In scirrhosity of the womb, we sometimes find that the whole substance of the uterus, together with the parts of the vagina which lie contiguous, are involved in a scirrhous disorganisation of the *diffused* kind, spreading itself equally in all directions through the uterine substance, and under this diffused scirrhous disorganisation the uterus may enlarge gradually and greatly in its size. At first, perhaps, it becomes as large as the pullet's egg; then big as the closed hand; afterwards bulky as the child's head; and, ultimately, as large as the womb, at the end of a nine months' pregnancy, or even larger than this, as you may see in the preparation before you. In other cases, again, in place of this diffused scirrhosity, the parts are assailed with a scirrhosity of the *tubercular* kind; and here, perhaps, in the first formation of the disease, the general structure of the uterus is perfectly healthy to all appearance, but embedded in its substance are the tubercles, sometimes fewer, sometimes more nu-

merous, ten or twenty in number, or perhaps not more than one or two. At first these tubercular scirrhi are not larger than peas, but within limits they may enlarge very considerably, so that when the tubercles are numerous, the womb, in this variety of the disease, may become as large as in cases of diffused scirrhosity, as indeed the preparations before you demonstrate. There is yet a third variety of this affection, and which, in practice, it is necessary to distinguish from the two former, and it is that in which you have a *single tubercle* only, or only one or two tubercles of large size, the rest, if any, being of diminutive bulk, so as to have but little influence over the symptoms of the disease. When the large tubercles are few, or single, they may be variously seated in different cases, on the fundus, the mouth, the front, the back, the sides, the womb sometimes enlarging exceedingly under this form of the disorder, indeed no less so than in the two preceding varieties.

In these cases of single tubercle, very much depends upon the seat of the disease. If the indurated mass be growing from the fundus of the uterus, or laterally, it may occasion but little inconvenience, but it may distress the patient much when seated in front or posteriorly, as in these situations, more especially if it lie low in the pelvis, it must urge and distress either the bladder or intestine, so that the patient and the practitioner are led to suspect a variety of diseases, as, for example, hæmorrhoids, cancer of the rectum, ascarides, calculus, strictures of the urethra, irritable bladder, and many other affections, of which the enumeration would be tedious.

Here, then, are three varieties of the disease, all, perhaps, essentially the same, but, in a view to practice, all properly distinguished from each other—the diffused scirrhus, the scirrhus with many tubercles, and the scirrhus in which the tubercles are few. And to this let me add, that in all the three varieties of the disease, but in the diffused scirrhus more especially, it is not to the womb only, or to the womb and the vagina, that the disease is confined, for it not unfrequently happens, that, together with these parts, the ovaries, the tubes, the bladder, the rectum, nay, in rarer cases, the liver and lungs themselves, are involved in the disorganisation; understand further, that the disease is more likely to prove topical when it takes the form of tubercle, and more frequently spreads over the parts contiguous, when it appears in the diffused variety. Let us now observe the illustrative preparations.

Here is a specimen of the os uteri beginning to enlarge, under the diffused scirrhus.

Here is another specimen of the disease, in which the scirrhous change is of the diffused kind; the surface of the uterus, you may observe, is rounded, and very smooth and equable, very different from what we observe in cases of tubercular scirrhus in the advanced stage, for in them the uterine surface becomes irregular and tuberose, bumpy, if I may be allowed a coarse but intelligible and significant expression—a state of tumour which is the rather deserving of your notice, because it is very strikingly characteristic of the disease.

The specimen which I next exhibit, is of single tubercle growing from the posterior surface of the uterus, and which would have ultimately troubled the patient much, by bearing on the rectum and the parts adjacent.

The preparation which follows, is a specimen of many tubercles; the disease is here in the incipient stage, but the womb must, at length, have enlarged to a great bulk, if the patient had lived sufficiently long.

Here is a specimen of a solitary tubercle of great bulk, formed on the womb anteriorly, and close by it stands a beautiful specimen of the diffused scirrhous disorganisation of a rounded and equable form, the whole mass being about as large as the womb at the end of a pregnancy of four months.

Here is a specimen of the diffused scirrhous action, presented by my friend Mr. Workman, of Reading; it has acquired the size of a nine month ovum. In this case there was a sort of fungous growth pushing forth into the vagina, and giving rise to very frequent and copious bleedings, under which the patient eventually sunk. The next specimen is an interesting example of polypus concurring with the scirrhous disorganisation; and this leads me to make another remark respecting the morbid anatomy of the parts, which is, that in scirrhosity the disease may consist of scirrhus merely, or of scirrhus in conjunction, with polypus, fungus, or other affections, in themselves more or less formidable, though these combinations are by no means very frequent. In the preparation circulated, you may also observe that the state of the os uteri varies much in this disease; it may be large or small, or with contracted, or with capacious opening, or with induration, or with softening; so that, although a scirrhosity of the os uteri, and of the vagina, is a strong corroboration of the real existence of reputed scirrhus in the parts above; the contrary is not certain; and we must not hastily infer, because the mouth of the womb is healthy, that, therefore, there is the same soundness in the parts of the womb which lie above.

Of the Characters of Indolent Scirrhus of the Uterus.—The characters of scirrhous uterus are conveniently enough divided into those which manifest themselves when the disease has been of long standing, and the womb has acquired a large size, and those characters which are observed in the earlier stages of the scirrhous disorganisation. Now you will sometimes find patients coming to you with abdomens as large as if they were in the end of pregnancy; but, on a little investigation, they tell you that they have been labouring under the disease for years—a clear proof that the enlargement is not from gestation, for with extra-uterine pregnancy it is scarcely worth while to embarrass ourselves here. On further inquiry, you discover that it is not in the upper but the lower part of the abdomen that the disease was originally seated; and, therefore, that the intumescence does not arise from an enlargement of the spleen, liver, or omentum, but of some part below; probably, therefore, the womb or ovaries, for an overgrowth of the kidney is exceeding rare. You will find, too, if you examine with great care, that the tumour is lying in the place of the gravid uterus in the general, not obviously inclining more to the one side than the other side. If the disease be of the tubercular kind, and of many masses, the uterus will have the tuberose form and feel; and if it be a diffused scirrhous disorganisation, as in Mr. Workman's preparation, then the feel of the uterus will be precisely the same as it is at the end of pregnancy, except that it is harder. Well, then, if you find in the abdomen a tumour hard, circumscribed, of years' standing, and which, therefore, cannot be referred to pregnancy, you may be pretty well satisfied that it is from gestation; and this opinion will acquire additional strength, provided you learn that the tumour formed originally in the region of the pelvis, and that it still lies centrally in the region of the gravid womb. Tuberosity of the swelling is a useful corroborative diagnostic, but a smooth and equable surface is no disproof of the disease. An indurated os uteri is a valuable sign of scirrhosity, but the womb may be scirrhous, although its mouth be sound.

Patients labouring under scirrhus of the uterus, will sometimes call upon you much earlier; when the tumour is not bigger than the fœtal head, for example, in which condition it may produce many distressing symptoms, although the tumour may not be very obvious to the touch. Now, in these cases, perhaps the patient tells you that there is a great deal of bearing, as if the interior would push forth; and she adds, perhaps, that she has no small irritation in the bladder, and pains of the hæmorrhoidal kind; and there may be ripping or lancing pains along the limbs, though these are not common; and there may be a want of muscular power in the legs, with numbness, and other marks of pressure upon the nerves. As the inquiry proceeds, perhaps you are told that there is some swelling seated above and behind the symphysis pubis, perceived as the patient lies in bed, compared, perhaps, to the fœtal head, or to a melon. Now when you meet with one of these rounded tumours in the region of the bladder, combined with irritation of the rectum, and bearing, and the other symptoms enumerated, you may always entertain a strong suspicion, that the patient labours under the disease we are now considering; but it is by examination, and by examination only, that the affection may be made out with certainty; and where the importance of the inquiry is deemed sufficient to counterbalance the inconvenience, you ought to proceed exactly in the same manner as in investigating a reputed pregnancy of four months, but directions on this point I shall presently communicate. If you feel the uterus hard and round, and large as the fœtal head, there can be little doubt respecting the nature of the affection; if this mass be tuberose, the proof is still more decisive; and if the os uteri is hardened, or if the vagina be thickened and indurated, as you may observe in the preparations sent round, there can be little doubt as to the real nature of the disease. Remember, however, that you must not decide too hastily, and you must not infer that you have a scirrhosity of the uterus, merely because the uterus is enlarged, for this enlargement may arise from a pregnancy of four months, not always disclosed to the practitioner; it may arise, again, from a pregnancy of seven or eight months; and then the abdominal muscles, and the uterus, being very thin, you may feel the head with considerable distinctness, and may mistake this for a scirrhous womb.

Let me add, too, that in some of these cases the womb prolapses a little, and no os uteri can be felt as such, the aperture being more contracted than ordinary; and, in such circumstances, if the uterine enlargement be inconsiderable, the case may be mistaken for a chronic inversion, an error of which I have seen two instances. Errors like these, however, imply a want of due skill, or due attention in the practitioner. A blunted probe may, in these cases, be passed into the uterine cavity. If, however, you bear these errors in mind, and if you examine with due care, repeating the investigation, if necessary, at the end of one or two months, provided you possess the requisite dexterity, your diagnosis may, I think, generally be established, and with certainty enough.

In all varieties of the disease, whether of

single tubercle, of numerous tubercles, or of scirrhus of the diffused kind, there may be mucous discharges or flooding, or occasionally a regular flow of the catamenia. Frequently there are inflammations of the scirrhous mass; sometimes there are ulcerations, but I add with satisfaction, that those ulcerations, always formidable, *are by no means frequent*. Occasionally, the urine is retained, and this, in cases of the tubercular scirrhosity, especially where the growth is in front; occasionally violent spasmodic pains are felt in the uterine region.

Treatment.—The treatment of this disease, so far as it admits of that which is useful, may be dismissed in very few words, for, in truth, there is little to be done; yet some remarks may be made with advantage. And, first, I should dissuade you from having recourse to any very active remedies, in order to occasion an absorption of the scirrhous deposit. Iodine may be thought of, but much purging, copious doses of mercury, conium, and other remedies of the active kind, should not be employed in this view; for there is no reasonable hope whatever of removing the tumour in this manner, and if you injure the constitution by this rough practice, you leave the patient worse than you found her. Although, however, you may lay it down as a rule, that you have it not in your power *medically* to produce an absorption of the scirrhous matter; yet there seems to be little doubt, that such absorptions may occur spontaneously. By a very excellent writer and practitioner, Dr. Clarke I mean, a case of double tubercles is recorded, in which the masses wasted away, and the patient died under another disease, when, upon examining the uterus, there were the traces of the tubercles still to be seen, so as to prove clearly their previous existence in larger bulk. This shows, that now and then the natural power is capable of accomplishing an absorption of the deposited matter; an encouraging fact, though certainly of very rare occurrence. Let me add further, that with scirrhosity of the uterus in its less extensive forms, impregnation is not impossible; and as the whole absorbent system is developed in the uterus by gestation, and afterwards excited powerfully on delivery, perhaps, now and then, a removal of the scirrhous material may be effected in this manner; and I once met with a case which, to me, appeared to be of this kind, but as it occurred in my earlier practice, and was not investigated with sufficient nicety, I would have you receive it as apocryphal; for I should be sorry to add to the huge and cumbersome mass of pseudo-phenomena with which the whole healing art is overwhelmed. What you are to do, therefore, in cases of this sort, will turn principally on palliation, and leeches, fomentations, and the antiphlogistic plan of the milder kind, may be proper when inflammatory symptoms occur; but where there is this extensive disease in the abdomen, I would not advise you to bleed largely from the arm. If there is, as sometimes will be, much spasmodic pain in the uterus, leeches and fomentations, and abdominal poultices, may be looked upon as useful palliatives; and after these have been premised, you may have recourse to anodynes, either taken into the stomach, or used in the form of a suppository. Above all, when there is much irritation of the bladder and rectum, I would recommend you to ascertain whether the enlarged uterus is retroverted or prolapsed, for this is sometimes the case. The uterus prolapsing, may descend a great way towards the orifice of the vagina, and may in that manner obstruct the bladder. Where this occurs, perhaps a pessary should be introduced, at all events the uterus should be passed above the brim of the pelvis, and after this operation has been performed, the urinary passages may become pervious, so as to render the use of the catheter easy, or perhaps unnecessary. When the womb is retroverted, you may empty the bladder and replace the uterus in the same manner as in the retroversion of pregnancy, an operation which has already been explained at large. In the general, remember that the less we interfere with indolent scirrhosity of the uterus, the better it is for the patient, and, above all, beware of salivations.

Prognosis.—The prognosis of this disease you may look upon as favourable upon the whole; for though it rarely happens that women are cured of this disease, yet it is not, I think, often, that it speedily destroys life; so far, therefore, the prognosis may be deemed very favourable, compared with that of many other diseases of disorganisation. But although this disease does not commonly destroy life, at least in a short space of time, it greatly inconveniences patients by its bulk, its weight, and the displacement of the womb by prolapsus, retroversion, and pressure of the enlarged uterus upon the nerves and other parts. In some cases, too, the patient becomes obnoxious to floodings, particularly if there is a fungous growth, and these floodings may shorten her life. Occasionally too, inflammations occur, indeed they are by no means uncommon. Sometimes, *though rarely*, I believe, malignant ulcerations manifest themselves, and by them she may be destroyed. Add to this, that the disease may spread into the bladder, rectum, or parts adjacent; and I have known it lay open the rectum into the peritoneal sac, the fæces entering there, and suddenly destroying the patient with collapse, as from internal hæmorrhagy. The body was inspected after

death. Remember, too, that the disease may be accompanied with other affections, as polypus, or spongy or fungous growths, well supplied with blood, not to mention the concurrence of hepatic tubercles; so that, on the whole, though the prognosis is favourable, and though the patient is not generally destroyed, or, at least, not speedily by this disease, which may be long protracted; yet it is not without great evils, and now and then the patient perishes unexpectedly by floodings, by ulcerations, by openings into the rectum, and occasionally, perhaps, by the extenuation of inflammations.

In scirrhus of the uterus, so long as the symptoms are not urgent, so long it is scarcely fitting to expose the patient to the inconvenience of examination; but should it be deemed necessary in doubtful cases to investigate thoroughly the state of the disease, a manual inquiry must then be made. It is true, indeed, that such surgical operations may be by no means agreeable to the feelings, but circumstances may be urgent, and it may be necessary to submit. And we will, therefore, in closing the Lecture, give a little further attention to this subject, commencing with the consideration of those cases in which the disease has made considerable progress.

When a patient is labouring under a reputed uterine scirrhosity of long standing and large bulk, one of the first points to be determined is, whether the abdominal bulk arise from air, water, adeps, or a diseased growth of the viscera, or from two or more of these causes combined. Gaseous enlargements are elastic, and yield exceedingly under well-directed pressure, and may be readily urged from one part of the abdominal cavity to another. The enlargements of dropsy may, in general, be recognised by fluctuation, more obvious when the fluid is in the peritoneum, more obscure when it is encysted, whether in the bladder, ovary, or the womb, distinguishable, however, in most cases, unless the sac be unusually thick. Adipose enlargements are not confined to the abdomen generally, but affect equally all parts of the body, and may, therefore, be easily recognised; so that if on examination we find a large abdomen, firm, hard, and unyielding, and not to be attributed to accumulations of water, air, or adeps, there is good reason for believing that the intumescence is arising from some *solid* growth; the abdominal mass consisting of solid material only, or of some large solid substance, in which a fluid may lurk.

Again. When satisfied from examination that the abdominal intumescence arises from solid growth, we ought next to ascertain whether this growth be seated in the upper part of the abdomen or inferiorly. In women, solid enlargements of the liver, spleen, or omentum (unless adipose) are not common; yet, when they exist, lying in the superior and middle regions of the peritoneal cavity, they are very readily distinguished by their firm, hard, and unyielding character, and by our being unable to make a deep depression when the hand, applied in the region of the hypochondria, below the margin of the ribs, is pressed perseveringly towards the spine. Should the swelling be uterine and scirrhous, of course it will occupy the lower and middle parts of the abdominal cavity, not reaching the pit of the stomach till the disease is advanced to its last stages; and hence, on pressing the parts which lie in the scrobiculus cordis, we shall find that they yield readily under the action of the hand, dexterously applied, while the central parts, and those below, possess an unyielding solidity, even firmer than that which is produced by gestation. It being ascertained, then, that there is a solid enlargement in the lower and central regions of the abdomen, it still remains to be known whether this growth be uterine or ovarian, or a combination of the two affections, a point which, in some cases, it is not very easy to decide. In general, however, when the enlargement is uterine, the mass will be found to lie equidistant between the wings of the ossa innominata; but should the intumescence chance to be ovarian in its nature, then, on *careful* and *dexterous* investigation, it will, I believe, in most instances, be found lodging more upon the right side than the left. Thus, then, by examining manually, we may, in most instances of scirrhous uterus, ascertain that there is a large solid growth in the uterine region, of many years' standing, as we learn from the patient; and where this is the case, there can be but little doubt that this enlargement is of the nature of an indolent scirrhus. It must be admitted, however, that the proof is not decisive, as, not to mention pregnancy, the womb may, perhaps, enlarge from other causes; but these are of rare occurrence, and I think I may add, that, in the present state of knowledge, these would not prove of much practical importance. If large tubercles can be felt through the abdominal covering, this will greatly help the diagnosis; if the mouth of the womb and the vagina feel scirrhous, there can be little further doubt. Scirrhosity of the womb and of the ovary may be combined. By emptying the bladder and lubricating the abdomen, and placing the patient recumbent, and relaxing the abdominal muscles, the investigation may be much facilitated. The whole inquiry, however, requires tact.

But what is to be done in those cases in which we are called upon to decide respect-

ing the existence of uterine scirrhus, in the first stages of its supposed formation, when, for example, the mass is no larger than the head of the fœtus? Why, in cases of this kind, the bladder previously suffered to become distended, may immediately, before the examination, be emptied of its contents, in order that the abdominal muscles may become more completely relaxed; and then the patient may be placed on the left side, in the ordinary obstetric position here demonstrated, and one or two fingers of the left hand may be laid upon the mouth of the womb, while those of the right are applied immediately above the symphysis pubis, in the region of the bladder, where the upper hemisphere of the enlarged uterus may be felt; so that the womb being interposed between the two hands, its bulk and firmness may be ascertained with nicety. In rarer and anomalous cases, the index of the left hand may be placed in the rectum against the back of the scirrhous uterus, while the thumb is resting on the uterine mouth; and the right hand being applied, as before, above the symphysis, the state of the womb may be investigated even with greater exactness than before. By these means, a competent and dexterous examiner may almost always ascertain whether the womb is enlarged or not, and the more easily if the patient have borne children; and if, as often happens, in cases of scirrhosity, there has been more or less wasting of the flesh. In making these examinations, if tubercles are felt through the abdominal coverings—or if the os uteri be large and hard—or if the vagina be scirrhous, of course in this, as in the former case, these diagnostics are of no small importance in marking the character of the disease. In doubtful instances, the bulk of the uterus may be registered after a first examination; and a second inquiry may be instituted some two or three months after the first, so as to ascertain what is of no small importance in the diagnosis—I mean, whether the enlargement be tardy or rapid in its growth. In cases of this kind, it is not to be forgotten that the uterus may enlarge, not from scirrhus merely, but from a pregnancy of four months—from hydatids—from mole—from polypus—and further, that an incompetent investigator may feel the head of a fœtus, mistaking this, perhaps, for a large and scirrhous uterus, for of this error, as observed before, I have seen two examples. Remember, however, that if the enlargement is from a pregnancy of four months, or from hydatids, it will increase rapidly—if from the head of a six or seven month fœtus, the case will be speedily developed by delivery; or if, lastly, it depend upon mole or polypus, sooner or later these substances will most probably be expelled; and, in the mean time, if we follow the treatment before advised, our error will lead to little practical inconvenience. Recollect that affections may be combined, and uterine scirrhus may be associated with polypus, pregnancy, and various affections of the ovary, to be considered hereafter; and let your opinions be given, not indeed with the mean and paltry reserve of ignorance, aping knowledge, but with that philosophic caution which the essential obscurity of the case may require. Gross errors are sometimes committed in these matters, but these are frequently rather to be attributed to the artist than the art. We ought not, however, to undervalue our brethren, merely because they fail in this part of obstetric knowledge; a man may be a well-informed, judicious, and, of course, a very valuable practitioner, and yet he may not have been in the way of acquiring that nicety of tact, which alone can give worth to his opinions in inquiries of this kind. (Dr. Blundell here gave the demonstrations.)

LECTURES

ON

MUSCULAR ACTION, AND ON THE CURE OF DEFORMITIES.

By Mr. SHELDRAKE.

On Artificial Muscular Action, and its Uses.

Having endeavoured to show what muscular action, involuntary and voluntary, is, I shall now proceed to treat of artificial muscular action; by that term I understand muscular action that is effected by some external agent, without any participation, or effort, being made to assist it by the person whose muscles are acted upon; but, to render it really useful, it is indispensably necessary, that the person whose muscles are acted upon should make no resistance, or effort of any kind, nor in any way counteract those exertions which are made by the operator who undertakes to direct the use of this exercise.

The discovery, if it deserves that name, of this practice, and the beneficial effects that I have been, for many years, able to produce by it, I made by reflecting on one of the observations of John Hunter,* and, acting upon that observation, till I acquired the power of curing distortions of the feet; of these, several remarkable cases have been

* See The Lancet, No. 268.

inserted in the last volume of THE LANCET. As I have practised this treatment with very great success, and very extensively, for more than forty years, and many of my early patients are still living, in good health and in the full possession of their natural forms, no doubt can now be entertained of the efficacy and the value of that discovery. In this practice I have made important improvements during the last twelve years, and shall communicate them through this channel, at some future opportunity.

I have applied the same principles of muscular action, both voluntary and artificial, to the cure of spinal curvature, and the numerous peculiarities in the human form that are connected with it, and with success quite equal to that with which I have been so long enabled to cure distortions of the feet; indeed, the cure of both these classes of defects depends upon similar applications of the same principles; but, as spinal curvature is connected with some peculiarities which require separate investigation, I shall postpone the consideration of that subject, and those that are connected with it, till I can bring the whole together under one point of view. I shall, at present, confine myself to showing, that the artificial muscular action, which I have employed in curing distortions, may be employed with much advantage to assist the efforts of medicine and surgery in the cure of some diseases, for which additional powers are wanted to effect all the benefits that such cases require.

Every professional man has, at some time, occasion to tell his patient, who is either invalid or convalescent, to take some gentle exercise, but to be careful that he does not over fatigue himself, as that will produce debility, and, therefore, will be injurious. No one can doubt the propriety of this advice, yet it is often unsuccessful, because a patient is, sometimes, in such a condition that he cannot take even the smallest exercise, without feeling effects that, instead of being beneficial, diminish the small portion of strength he has, and which exercise is, therefore, highly injurious.

If an invalid has a very small portion of health, a great quantity of prudence, and acts with great perseverance, he recovers his health, though but very slowly, because the exercise itself produces fatigue; this is well known to all who, being ill, are unable to move about, except they are forced to do so. If the first principles of muscular action, which I have endeavoured to develop, are referred to, it will be seen that every time a leg, or other limb, is moved, all the muscles that are employed to raise and carry it forward are made to exert themselves, which, in those who are weak, soon produces fatigue.

The great benefit which is produced by exercise is, that it increases the vigour with which the blood circulates through the whole frame; that increased circulation promotes all the natural secretions, promotes the absorption and deposition of the material that is obtained from food, which is thus converted into animal matter; this process increases the size and strength of the person by whom such exercises are practised.

One of the plans that I have adopted, is to increase the quantity of beneficial exercise, which any person may take, *without any exertion of their own*, and of course without fatigue, and is effected in this manner;—I have invented a chair, in which my patient is seated; if it is intended to act upon the arms of the patient, two cranks are connected with the chair, in such a manner, that when the patient places one hand upon each crank, so as to keep the handle in her hand, but without any exertion, the machinery that is connected with these cranks is so arranged, that, when put in motion by an assistant, the arms, together or separately, as it may be desired, are made to describe circles of any diameter, or to move in any direction that the circumstances of the case may require.

One advantage of this invention is, that the assistant, who turns the handle, does all the work, and the patient, who sits still in the seat, receives all the benefit, because, by keeping her hand, or hands, upon the handles, the whole of the arms, shoulders, scapulæ, and pectoral muscles, may be kept in action for any time, or to any extent, that can be required, without any exertion of the patient, or any fatigue whatever.

Another advantage of this invention is, that the movement may be either extremely slow, so as to adapt it to persons whose very delicate health will allow only of the most gentle exercise, or it may be increased gradually, as circumstances require; or it may be given at once to any extent that may be desired, and, in all cases to which it is applied, the full effect of any quantity of exercise may be given without producing any fatigue whatever. It must, necessarily, be deduced from this fact, that in any case in which muscular exercise can be beneficial, the full physical effect of such exercise may be produced upon any person, and not be attended with any inconvenience whatever.

As this is the first time that this application of my discovery has been publicly offered to general notice, I trust that I shall be excused for retracing its progress from its origin down to the present time, before I show, more particularly, its application to the subject that is now under consideration, as well as some others to which I am

aware that it may be applied with much advantage.

More than forty years have elapsed since I began my experiments upon this subject, those experiments that were founded upon a fact that had been discovered by Mr. Hunter. That greatest of surgeons saw and approved of those experiments, and likewise recommended many of his own patients, to whom my treatment was useful. Mr. Hunter watched the progress of his own patients, and was satisfied with what he saw. Many other surgeons, of equal rank in society, honoured me with their notice and approbation, and this practice was adopted to a great extent, privately, under their observation, and my practice went on with success. After the intercourse that I had with Lord Byron, the particulars of which are detailed in THE LANCET,* I made very great improvements in my treatment.

At last, I determined to make the general principles of my discovery public for the general use of society at large, as well as of those members of the medical profession who might choose to adopt the practice; in consequence of that determination, I wrote to the editor of THE LANCET, with whom I had no acquaintance whatever, that letter which was first published in his Journal, and those which followed it in the last volume. Much interest was excited in the minds of professional men, as well as others, on the perusal of the cases; but some doubted their correctness, and even denied the possibility of performing those cures, and many expressed a wish to see them when they occurred. In consequence of this, when the case of Robinson came into my hands, I inserted a brief statement of it in THE LANCET for November 29, with a request that all, or any gentlemen who might be desirous to see this case, might do so at any time they would themselves fix, during the whole progress of the cure. As I have the satisfaction to know that this offer was very favourably thought of, and the case has given much pleasure to many gentlemen who have examined it, I take this opportunity to repeat that the offer remains permanently open during the whole time of the cure, whatever that time may be. Having done this, I shall not again allude to the cases that were published in THE LANCET, before that offer was made; but, in any thing that I may have occasion to illustrate, I shall refer to the case of Robinson, and some others; because, whatever I may have occasion to say upon the subject on which I am now writing, I can demonstrate to be true, by reference to the cases which are daily in my hands. In the case of Robinson, three distinct processes are now going on, but are all being produced by the effect of one operation. First, the positions of the bones of the leg and foot which are deranged and placed in unnatural positions, are being restored to their natural situations; secondly, the ligaments are arranged in their natural proportions relative to each other, and are regularly and gradually resuming their natural powers and functions; and, thirdly, the muscles which were rigidly fixed, without having power to act in any manner, are now resuming their natural proportions relative to each other, and to the bones and ligaments with which they are connected, as well as resuming their natural functions.

When the cure of this case is completed, I shall give full details of the treatment by which it will have been effected; but, at present, I shall confine myself to showing so much of the alterations that have taken, or are taking place in the muscles, as will enable me to illustrate that particular kind of muscular action which it is now intended to explain.

It is known to all, that exercise is one of the best means that can be used to restore the health of invalids or convalescents; but it has been very imperfectly known, or at least noticed, that exercise, in other words, muscular action, which is produced by the will of a living being acting upon its own body, is necessarily accompanied by exertions that produce fatigue, and, as a necessary consequence, when it is carried to excess, injures the health of that body it was intended to serve, and to improve. But, I believe it has not been known to any but John Hunter, and those who may have derived their knowledge of this fact directly from him, which I cheerfully confess that I did, that when some parts of a living body have lost their power of acting in obedience to the will, if those parts are forced to repeat their natural actions by extraneous means, over which the person who is operated upon has no control, the natural power of acting, in obedience to the will, will be resumed by those parts.

This very extraordinary fact has, I repeat it, been unknown to all, except those that have been mentioned; yet it has been proved to be true by the whole of my practice in all these cases, and may be seen going on, at the present moment, in that case which I have offered for examination. As this effect is produced by means which require no exertion to be used by the patient, and which are in no way under his control, they do not produce any fatigue in his person, for whatever time this exercise is continued, and to whatever degree it is

* See THE LANCET for 1828, vol. ii., p. 779.

given; by this means health, strength, and increased size, are produced in those muscles which are subjected to this exercise, as may be seen in the case of Robinson. When my treatment of his case first began, his foot and leg were quite rigid, and wasted so as to be of the greatest paleness: in a few days some colour began to appear about the toes and heel, extended gradually upon the foot and leg, so that now, at the end of two months, the foot is not only improved in form, but is increased in size, substance, and in colour; it has all the appearance of high health; this healthy appearance extends upwards about a third part of the leg, above which the original paleness remains; but the healthy colour is gradually encroaching upon it.

These facts can only be accounted for thus. The action into which my operation forces the foot, increases the circulation of the blood, and gradually enlarges those vessels which had been so contracted, that the red particles of blood could not pass, till a free passage is obtained for them, and by this means the bulk, as well as strength, of the leg is in the course of being restored: this is certainly the case, for the circumference of his leg has been measured within a few days, and found to be, at the largest part, one inch larger than the cast that was taken from the same leg two months ago.

This patient has told me, without being asked for the information, that, before he came to me, his leg and foot were always very cold, but now they are constantly in a pleasing glow, similar, and quite equal to that of his other foot; of this peculiar effect, I will give another striking example.

After I had published in THE LANCET for November 29th, my invitation to gentlemen of the medical profession to examine the case of Robinson, a gentleman, who has a daughter with a foot distorted in a form that is very like that of Robinson, sent his medical adviser to examine that patient, although he lived at the distance of one hundred miles from London. This gentleman, accompanied by two medical friends, came to my house, examined Robinson's case with great care, and concluded that I should certainly cure him; by their recommendation the young lady, upon whose account they came to London, has been placed under my care. She is six years old; her foot is very like Robinson's; many unsuccessful attempts had been made to cure it; in consequence of what she had suffered from these attempts, she was very timorous when my treatment began; but, on the third day, without being asked questions, she said she saw colour coming into her toes, and felt that her leg was warm instead of being cold as it had been, and this effect has continued to increase, so that now her leg has the same genial warmth as her other leg, besides being greatly improved in form.

This child's foot was as much distorted as that of Robinson, yet I have produced as much alteration in its form in one week, as was produced in Robinson's by a constant attention of two months; this fact will show the great advantage of attending to these defects at the earliest periods of life, while that of Robinson, and others that have been detailed, must afford consolation to those who are farther advanced in life, as that circumstance alone does not render them incurable.

The inference to be drawn from these facts, as applicable to the subject now under consideration, is, that muscular motion, when artificially produced by the application of means over which the patient has no control, and which do not require any exertion on the part of the patient to produce, does, according to the declaration of Mr. Hunter, restore the natural connexion between that mysterious power, volition, and the muscles upon which it is natural for it to act; it likewise increases the size, health, and strength of muscles, to which it is carefully applied, after they had been reduced by illness, as well as other causes, to a state of great debility. This fact has been proved by the success of my practice upon this subject, particularly in that case which is now in progress, and is laid open to general observation.

But, it may be asked, in what does this treatment differ, either in principle or in practice, from those modes that have been previously used to produce similar effects? The short answer is, that it *is* superior in efficacy to those practices, when they have been in any way serviceable, and has been completely successful in other cases, when the former practices were quite useless. It will, however, require some time to demonstrate these facts; yet, as the subject is important, I will do what my experience enables me, to establish the superior efficacy of this practice.

The means that have been employed, independent of medicine administered by the stomach, to restore such limbs as I have described, to their natural powers, are electricity, frictions, embrocations, with oils and other mixtures, champooing, and, finally, blisters; all these remedies may be useful in cases to the nature of which they are duly proportioned; but, in such as are now under consideration, they are perfectly useless. I have invariably found that they are so; but, as a strong prejudice exists in favour of their efficacy, in such cases, both in the minds of some professional men, and of other persons of a different description, it will be, in some respects, beneficial to produce authentic

facts, which will show that these practices are entirely useless.

Before Robinson became my patient, he was for many months an out-patient at the Middlesex Hospital; he was generally, if not entirely, under the care of the late Mr. Shaw. In consequence of that gentleman's directions, the patient kept his leg and foot for a long time together over the steam that exhaled from a vessel of boiling water, and was directed to keep it so as to make the foot and leg as hot as he could bear them to be; this, it was said, was intended to soften the skin, the muscular fibres, the tendons, and capsular ligaments, and qualify them to receive the oils which were afterwards to be applied in order to render them still softer.

He was furnished with a quantity of oils; he was not told what they were, but he was directed to have them rubbed all over his leg and foot with great force for several hours together, that they might very effectually soften his leg; the smell of these oils, he said, was very offensive, and their effects were irritating; that his mother, who operated as rubber, had her hands so completely excoriated, that she was often obliged to desist; and, as no advantage was gained by the application, after a painful perseverance of more than six months, she threw them aside.

It is well known to the profession and to others, that ointments and embrocations of various kinds may be advantageously employed, when they are properly proportioned to the complaints for which they are used; as mercurial ointment in syphilis, and many others upon different occasions, which it is not necessary to mention here. Mr. Shaw, likewise adopted a practice with this patient, which, I believe, is quite original, and so extraordinary, that I should hesitate in ascribing it to him, if the youth was not at hand to establish the fact. As this was a secret worth keeping, as well as worth knowing, and too valuable to be exposed to the profane eyes of the pupils who had paid their money to learn the practice at the hospital, to which the youth had applied as a patient, Mr. Shaw directed him, instead of going to the hospital, to go to his private residence in the Albany, while he lived there, and afterwards in Berner's Street, to which he removed. In these two hallowed temples of Esculapius, from which the profane uninitiated, either in or out of the profession, were carefully excluded, Mr. Shaw and his confidential assistant laid the patient at full length flat on his face; and while he was in that situation, Mr. Shaw rubbed the back of his leg, in its whole length, with a *brass roller, that was fixed in a long handle*, which enabled the operator to hold it with both hands, and bear on it with much force, while he rubbed it backwards and forwards with great velocity; this was repeated many times, for a long time at each operation, which always gave him great pain. These frictions with the brass roller always were very painful, never produced any advantage, although they were very sedulously repeated during a month, and at the end of that time he ceased to go to the hospital.

It is to be observed, that during the six months that this patient was under the care of Mr. Shaw, he was successively subjected to the action of steam, to dry frictions, to embrocations, which, I presume, are but frictions with another name, and intended to promote the absorption of stimulating oils, which the operator supposed would produce some beneficial effect upon the patient's leg and foot, though it is not easy to discover by what connexion between cause and effect *any* benefit could be produced by such frictions upon the leg and foot of this patient; at all events they showed that no benefit was to be expected from these frictions; this being the case, it cannot be understood why brass was adopted as the material to make this roller of, unless it was thought that metal had some specific virtue for curing a distorted leg, which the other modes of, and materials for friction, did not possess. Of the nature of that specific virtue we are quite ignorant, and cannot now be informed, because the operator is no longer in a condition to give the information.

I always believed, that Mr. Shaw was the sole inventor or discoverer of this specific virtue of curing distortions, but have lately discovered a competitor whose claim must not be lightly passed over.

At page 226 of a work published by Dr. Harrison, I have found the following passage:—"The prominent vertebræ and ribs were then pressed, and *driven* in the direction of their natural situations with an instrument held in the right hand. It has a wooden handle, into which is fixed at right angles, *a brass rod, four inches in length, and of strength enough to bear every degree of strength that the operator may deem it proper to apply.* To the lower end, *another round piece of brass metal*, about two inches long, is rivetted at right angles. This, well covered with soft leather to prevent its bruising the skin, constitutes, with the other parts, the instruments that I employ in all my manipulations."

Thus we see that Mr. Shaw and Dr. Harrison, who disagreed upon so many subjects, are agreed upon this, which seems to have no foundation in truth, viz., that brass metal has some specific virtue in curing distortions, as the first used it in the shape of a roller upon his patient, without produc-

ing any benefit; and the latter uses it in all his manipulations, to rub, pummel, or thump the ribs or vertebræ of young females, who are entrusted to his care; but what success *he* may have in that whimsical practice, is not generally known; at least no authentic and visible proofs of his success have yet come to my knowledge.

For authentic proofs of other unsuccessful practices that have been used to cure these distortions I must refer to another patient, who is at present under my care. About six years ago, a lady of mature age had an illness, of which I did not get a very satisfactory account. After her recovery she felt a little lameness in one of her feet; this went on increasing, till her foot turned inwards, so that she stood upon the outer edge of her little toe, the ancle-joint became quite rigid, as well as all the joints of her foot. When I first saw this lady, her foot bore a great resemblance to that of Robinson, but was not so much deformed.

During the progress of this distortion from bad to worse, the usual ineffectual remedies were resorted to; pumping upon the foot with cold water, steeping it in hot water, strengthening plasters, embrocations, dry frictions with the hand and the flesh-brush, &c.; as all these remedies failed, a severe course of blisters was adopted as a *dernier resort*; after bearing these for a month, without feeling any advantage, she discontinued them, with a determination to continue lame, rather than bear so much pain. The last summer she passed in Brighton, and used the champooing bath during her stay at that place, with no more advantage than she had derived from the other remedies, and, in October, placed herself under my care. At that time, her foot so exactly resembled the foot of Robinson, which is represented in No. 274, that I did not think it necessary to take a cast of it, but reference to that of Robinson will explain all the appearances of the lady's foot; the distortion was equal in degree and in rigidity in both cases; the principal difference between them is in the age of the patients, Robinson being not yet fifteen, and, as the lady has a son who is more than twenty, we may, without formally asking the question, venture to put her down as being forty years of age.

I have, on the present occasion, endeavoured to explain the general principles upon which the muscular action that I have used to perform these cures, operates to produce these effects; and, in my next, shall endeavour to show, that the same principles of muscular action may be advantageously employed to improve the health of invalids and convalescents, in the forms of whose persons there does not exist any defect, as well as in some other complaints, in which it may be used with advantage.

LONDON MEDICAL SOCIETY.

Monday, February 23, 1829.

SUBSERVIENCY OF THE SOCIETY TO THE COUNCIL.—POINT OF ORDER.—THE ALLEGED FEVER BRAIN CASE.—UNUSUAL FATALITY OF SCARLATINA.

The Council not having made their appearance at twenty minutes past eight o'clock, although several members and visitors were waiting in the Society's room,—

Mr. Shearly rose and stated, that by the laws of the Society, the Council were directed to meet between the hours of seven and eight o'clock, and that the proceedings of the Society were regularly to begin at eight o'clock. A considerable portion of time had this evening been frittered away, and he conceived the Council were not justifiable in thus acting; and, as the law stated that when seven members were present they were competent to commence the business of the Society, he should move, that some member, then present, should take the chair, there being neither President nor Vice-President in the room. He moved, that Dr. Johnstone should take the chair.

Dr. Johnstone would not take the chair; he could not think of such a thing; it would be indecorous in him to do so.

Mr. Shearly. I do not know how it could be considered indecorous for you to do so; it certainly is indecorous in the Council to keep us waiting here, wasting our time.

Dr. Johnstone. We do not know how they are engaged; let every man be judged of by his acts.

Mr. Shearly. But the Society is to meet and proceed to business at eight o'clock.

Dr. Johnstone. Well, but every one who is acquainted with these Institutions knows that such a circumstance as this will occasionally take place; it is as great a disappointment to me as to any body, that the business has not begun precisely at eight o'clock; but I think it is right we should first know what the cause is.

Mr. Shearly. There is a rule stating, that when seven members are present, we shall go to business, and I do not know why we should not proceed.

Dr. Ryan. No doubt the law is such; it is undoubtedly so.

Mr. Shearly. I do not think the Council have the power to keep us waiting; it would be perfectly inconsistent if they had.

Dr. Johnstone. I do hope and trust, and believe, it is not upon private concerns they are detaining us, but upon the concerns of the Society.

Mr. Shearly. But the Council have from seven to eight o'clock to deliberate, and surely the Society is not to be kept waiting for them. After they arrive, the Minutes of the last Meeting will have to be read, which will occupy from this time till nearly a quarter to nine, and then the Society will break up at half-past nine; now, what use is there in gentlemen leaving their business and coming a great distance to attend a Society of this kind?

In a few minutes afterwards the Council entered the room.

Dr. Williams took the Chair.

Mr. Shearly then said, Sir, I beg to rise to a point of order. The Society has now been kept waiting for twenty-five minutes. The business of the Society is to commence at eight o'clock. It has not been this evening only that the proceedings have been thus delayed, the practice has now become, generally speaking, the same every night; and I, for one, think it excessively indecorous that our time, so exceedingly short as it is, an hour and an half, should be thus frittered away before the business can begin.

Dr. Williams. Really, I must confess, this evening, the Society has been kept waiting beyond its usual hour, but a question of very material interest to the Society was referred to the Council, about a fortnight ago, and it has been under discussion this evening. That is the reason why the Society has been kept waiting.

Mr. Shearly. The Council are to meet from seven to eight, and I do not think it competent for them to keep the other members waiting here for twenty minutes, or half an hour, under any pretext.

Mr. Proctor. Perhaps Mr. Shearly will be satisfied when he is informed, that this question, of very great importance to the Society, is to be referred to him and the general body of the Members.

Mr. Shearly. No, I am not at all satisfied with that.

The Minutes of the last Meeting were read.

Mr. Shearly stated, that Dr. Blundell had considered that the specimen of monstrosity he (Mr. Shearly) exhibited to the Society on the last night, would be more useful to the public preserved as a specimen, than if it were dissected, and he had, therefore, presented it to that gentleman. This rendered it impossible for him to detail any morbid appearances. The twin was alive and likely to do well.

Dr. Johnstone. I believe this is the proper time to take notice of the Minutes, and though it does not appear by them that any notice was taken of a subject that was mentioned on the last evening, yet to that subject such publicity has been given, by a report, that I think it right to call on Dr. Ramadge to state, what is the "misstatement" I made respecting the case I then communicated to the Society; and who is the relative of the deceased on whose authority he has accused me of a misstatement.

Dr. Ramadge. Sir, I beg leave to say, that I have seen The Lancet, and that the statement I made in the Society has been correctly reported in that work. I never said that I had had an opportunity of seeing the relative of the deceased. I merely mentioned that I had seen several persons at the West End of the Town, and that it was reported there that the patient had fever. There is a gentleman in the room who heard Mr. Jewell state, that the patient had fever for at least five days before his death; that he had been in the country; had taken cold, and was seized with delirium. The gentleman who told me that he had fever was Mr. Howell, and I am sure he will bear me out in the statement. I did not state any thing intentionally offensive to Dr. Johnstone. My object was further inquiry into the case. I should be glad to know, whether this person had any discharge from the nostrils or not?

The Registrar. I did not insert Dr. Ramadge's observation in the Minutes, because, after the meeting separated, I thought Dr. Ramadge would have another opportunity of disposing of it, which I considered to be the better way. If any thing particular turns on what passed I can now make a note of it.

Dr. Ramadge. I took the only opportunity I had of making my observations. If I had suffered the Minutes to pass over then as they stood, I do not think I should have done right.

Dr. Johnstone. There are two things to be considered—the statement and the deduction. I have no objection to any gentleman differing from me in deductions; but I think, if a man differs from me in facts, he is bound to produce the evidence that warrants that opinion. I stated the symptoms, and I said there was no fever; I stated the condition of the skin, of the secretions, of the respiration, and so on, and I said, those did not exhibit symptoms of fever. Now Dr. Ramadge told the Society, for I came in late that night, and did not hear it, that he had it from a relative of the deceased's.

Dr. Ramadge. The statement never was made by me.

Dr. Johnstone. Then the only relative of the deceased was Mr. Jewell himself, who called on me on Saturday morning, saying, that he had read the statement in The Lancet, and he expressed his utmost surprise that it should have been made. He

not only authorised me to repeat, that what I had stated was true, but offered to come forward to prove every thing I have said. This gentleman had an opportunity of seeing the patient every day, indeed I may say every hour, and every week during the period he lived. The young gentleman was ailing for three or four weeks; for seven days before his death, he was out in the carriage, and from that time he was confined to bed, and then, I say, he had no symptoms of fever. I think it was really very unhandsome, to say the least of it, that Dr. Ramadge should have brought forward this subject behind my back; it was not only irregular, as I was not here, but I think, really, that knowing well that every thing against me is so exaggerated in The Lancet, (cries of hear, hear, order, and chair,) he ought not, then, to have addressed the Society on the subject.

Mr. Gosset was surprised at the time, that Dr. Ramadge should make the statement, and was more surprised at his not being called to order by the Chair. He recommended that the subject should be dropped, as he did not believe Dr. Ramadge made this statement with any intention to annoy, or to offend Dr. Johnstone, but from his not exactly understanding the regulations of the Society, he proceeded with it.

Dr. Johnstone. I am perfectly satisfied, and content that it should now drop.

Dr. Ramadge rose, but

Dr. Williams observed, I think it would be better that this matter should drop; however, I beg to bear testimony to the repeated expressions of respect on the part of Dr. Ramadge towards Dr. Johnstone, and though it may appear, that at that period he may have been irregular, I am sure that Dr. Ramadge did not intend any personal offence.

Dr. Ramadge. I appeal to the gentlemen who are here, and who were here at the time, whether I said that I myself had had it from a relative of the deceased? I mentioned that I had heard from a gentleman who heard from a relative of the deceased, that fever was not present; and what is stated in The Lancet is perfectly correct.

Mr. Wray brought forward the subject of scarlatina, to ascertain, if possible, the opinion of the Society on what produced death in some of the recent cases of scarlatina, where patients had sunk so speedily. He enumerated particularly two cases, where both the patients, two of the finest children he had ever seen in one family, died within about five days. In the post-mortem examination that he made, he had been able to detect nothing whatever in any of the viscera, to account for death; he therefore concluded, the nervous structure only was affected; and that death must have arisen from that. He lamented to find, that scarlatina was very prevalent at present, and extremely fatal, in spite of the most judicious treatment.

Dr. Ramadge had found scarlatina also extremely destructive of life. He was inclined to apply leeches to the throat in the first instance, followed by large poultices, to keep the bowels open, to sponge the body with vinegar and water, to make use of mercurial medicine and stimulants. He did not think gargles, in the inflamed state of the throat, of use; warm water was the simplest and best gargle. He likewise recommended a tea-spoonful of the infusion of roses, with a little syrup, frequently.

Mr. Procter had generally carried his patients through; and he thought scarlatina ought to be treated according to the strength of the patient, and very much upon the principle that would be adopted by a judicious practitioner in any other case of fever, where the nervous system was considerably affected. In some cases blood might, and in some it might not, be abstracted. The last season produced more fatality than usual in the neighbourhood where he resided. One very strong man, who had a sudden attack of jaundice, was bled, and soon afterwards became partly delirious; in less than 36 hours from the appearance of the disease, he expired. Dr. Chomelly had informed him, that five cases of the same kind had fallen to his lot, three of which he had carried through with stimuli.

Dr. Ryan had had considerable opportunity of witnessing scarlatina. In one house, two children died suddenly, the one five, and the other seven years old. Leeches to the throat, and treatment very similar to that recommended by Dr. Ramadge had been had recourse to. A third child in the same house became affected, which the parents refused to be allowed to be treated in the same manner, but gave wine, and applied sinapisms to the feet: he got well. There were cases in which it was impossible to save the patients. Stimulating treatment seemed to have been the favourite in ancient times; and he was inclined to resort to the decoction of bark, wine, and sinapisms to the feet.

Mr. Gosset had found great benefit from using the subcarbonate of ammonia.

Mr. Salmon regarded the chloride of lime in the shape of a gargle as a very excellent remedy; half a drachm of the chloride of lime to a pint of water, injected into the stomach. In some of the severe cases, the ulceration went down throughout the mucous membrane, not only of the stomach, but of the intestines; and he had

found injections of the chloride into the rectum exceedingly serviceable.

Mr. Ashwell had witnessed surprising benefit from using the subcarbonate of ammonia. He first attended to the state of the bowels, put the child into the warm bath, with some mustard, and immediately gave the subcarbonate, not confining himself to any particular form of it. He had frequently given 10 or 15 minims of the *spiritus ammoniæ aromatici*, in a little peppermint or sirup. He recommended light poultices, consisting of three parts of linseed meal and one of mustard, to be applied to the neck, by which he had seen children relieved from a state of almost certain suffocation. In more severe attacks, more active remedies were necessary; but he had not, for the last three or four years, employed leeches.

Mr. Brown had never employed the lancet to infants; he might have availed himself of the use of leeches. He objected to blisters remaining on infants, so long as to occasion much stimulating irritation, by which sloughing might be induced. He considered an ounce of the sirup of rhœdos, with 12 drops of muriatic acid, an excellent linctus for children. He had never given ammonia. He preferred the tepid to the warm bath. If he wished to give stimulants, he should prefer wine to ammonia.

Mr. Wray regretted that the object for which he had introduced the discussion, had not been attended to. He wished to have had the morbid symptoms, if any had been explained, by which death had been caused, and these had entirely been passed over. For his own part, he esteemed bleeding a very valuable remedy.

A Member, whose name we did not ascertain, expressed his opinion, that there must be something peculiar in the season, which alone could account for the great mortality that had prevailed of late in cases of scarlatina and other fevers. He had lost cases without being able to account for death in any other way. On dissection, he had discovered nothing.

CASE OF ST. VITUS' DANCE FROM AN EXTRAORDINARY CAUSE.

By Edward Harrison, M.D.

Mrs. Brabins, ætat. about 90, of a thin spare habit, was the mother of several children. She had through life enjoyed excellent health, the consequence of her temperate and methodical habits. April 20, 1812, she was seized with irregular motions of the superior extremities; they kept increasing for several days, when the inferior became similarly affected. Her attempt to perform any kind of work, and especially to convey meat or drink to the mouth, was attended with constant mistakes, which seldom failed to excite the laughter of bystanders, and in which she generally took her part. She could not direct her steps in going from one place to another, so as to proceed directly to the spot. One of the feet dragged, and made a sort of semicircular turn in walking. She continued in this state, with scarcely any alteration, till the following June, when she expired rather suddenly.

The next morning, my friend Dr. Veitch, of Horncastle, and I, were suffered to examine the head. On removing the skull, we found the dura mater looking flabby and very pale, as did the tunica arachnoidea and pia mater. There was an unusual quantity of moisture lying between these several membranes. The brain, when cut into, appeared extremely pale, but was of the usual consistence. On laying open the ventricles, we found them distended with a fluid, which was nearly pellucid. A large quantity of the same kind of liquid flowed freely, and, in great abundance, out of the theca spinalis. The chord and its membranes, as far as we could trace them through the foramen magnum, presented a similar hue to that of the cerebrum, and its involucra. No other morbid appearances were discovered, though we carefully examined the different parts of the brain and its membranes. As we were not prepared to meet with any preternatural quantity of fluid in the skull, or spinal tube, we incautiously suffered some of it to escape upon the floor. We could not, therefore, measure the quantity lodged in the brain and theca, but we were of opinion that it did not amount to less than six ounces, and probably considerably more. She retained her faculties almost to the very last; indeed, the day previous to her death, she was thought better; and it was, upon some sudden motion, either in rising from her bed, or from the night-chair, that she expired. She had always been considered (what is called) a nervous person; but from regular habits, of every description, she was unusually healthy, and remained free from complaint at her advanced years, till the period above alluded to; and she had not been in the habit of taking any medicine, excepting, occasionally, a little aperient electuary.

REMARKS.

The accession of idiopathic chorea after puberty is, as far as I know, of very rare occurrence. It is a disease of early youth, and generally vanishes on the full development of the constitution. Nosologists class it among the neuroses, supposing it to emanate from the nervous system. I have

not met with another instance, where chorea began in old age; at that period the nerves are too much blunted to suffer from chorea, under ordinary circumstances. We therefore presume to assert, that in the present instance the complaint did not originate in the usual way, but in the serous deposite, within the brain and spine. That extraneous matters lodged in the theca spinalis induce very distressing nervous symptoms, we know from the former and the following cases, as also from another published by Mr. Chevalier.* In the last, a girl, fourteen years old, complained of great anguish in her back, which was aggravated on sitting up. The pain kept increasing for several days. After an unusually severe attack, the mother thought she discovered a slight projection of the spinous processes, in the lumbar vertebræ. "I was sent for," says Mr. Chevalier, "but could discover nothing in the back that was not natural, nor was any particular pain excited by pressure. The effort to sit up distressed her much. On the following morning, to my great surprise, I was informed that, early in the preceding evening, she complained of a sudden and violent increase of pain, and immediately became convulsed; when the convulsions had continued five hours, she expired. I examined her body in the presence of Mr. Johnson, of Mortimer-street. There were no traces of disease in the brain, or in any of the thoracic or abdominal viscera. I found the cavity of the spinal canal, close to the seat of the pain, filled with extravasated blood, which, from its florid colour, must have been arterial, and which covered the whole of the cauda equina." The seat of the pain naturally leads to a supposition, that the effusion took place from a blood-vessel situated within the lumbar tube, where, by irritating the spinal cord, it excited the fatal convulsions.

(To be continued.)

GLASGOW ROYAL INFIRMARY.

WOUND OF THE POSTERIOR TIBIAL ARTERY.

John Buchanan, aged 12, was admitted under the care of Mr. Cowan (Jan. 21), with a wound situated over, and a little to the outer side of the right leg, equalling the size of a crown. An incision had been made directly afterwards, an inch and a half in length, and at its junction with the original wound, there was a ligature placed on some divided vessel. The wound was filled with coagulum, so as to keep its edges widely separate. There was also an oozing of arterial blood. The wound had been inflicted three weeks before his admission, and was followed by violent arterial hæmorrhage. It was at length checked by pressure, but not completely; for, during the succeeding 14 days, it had recurred twice. On the 17th January a ligature was applied, but scarcely with more success, for, as has been said, blood still continued to ooze from the wound on his admission. A pad was applied, so as completely to restrain the bleeding, and six drachms of the sulphate of magnesia given internally. The following day hæmorrhage to a considerable extent took place, from a vessel in the situation of the posterior tibial, which was secured. On the 28th, bleeding again took place, and on removing the dressings, the ligature was found detached. Since this occurrence, the case has gone on favourably.

* Med. Chirurg. Trans. vol. iii.

REMOVAL OF AN ADIPO-SARCOMATOUS TUMOUR.

Mrs. Kyle, aged 61, was admitted into the Hospital by Dr. Couper on the 25th of January, with a large pendulous tumour on the outer aspect of the superior third of the thigh. It measured eleven inches in length, and nineteen in circumference. The tumour itself was firm and lobulated, and was but loosely attached to the subjacent parts. At its most inferior part, there was an ulcerated opening of inconsiderable size, the centre of which was occupied by a brownish slough, and from this a thin sanies was discharged. The patient said the tumour was the seat of occasional pain, which was most severe and frequent during the night. The veins of the thigh and leg were varicose. Her health had also been declining for some time; her appetite had become impaired, her tongue white, and pulse 100. Her bowels were regular.

The tumour first made its appearance about sixteen years ago. At that time it was loose, and gave her no uneasiness; but four years after, when it had attained the size of an orange, it became affected with shooting pains, since which it had gradually, but constantly increased. A year before she came into the hospital, the skin broke at its most depending part, and, for three months, a thin ichorous fluid was discharged. It ultimately ceased, but afterwards ulcerated, and some time after three pounds of blood were lost, before bleeding could be checked. Since this occurrence, the shooting pains were more frequent and severe.

28. The tumour was removed to-day, by making a longitudinal incision on each side of its base, close to, and parallel with, the

thigh. Immediately afterwards, its cellular connexions were divided. Three or four tolerably-sized arteries continuing to throw out blood, were secured, and an attempt made to bring the edges of the wound together; but this, on account of the scarcity of skin, was found quite impossible. A suture was then had recourse to, which, being passed through the lips of the wound, was hauled to in a first-rate nautical style. The wound was afterwards dressed with adhesive plaster and a bandage. The tumour weighed five pounds nine ounces.

Feb. 7. The wound has been repeatedly dressed, and looks favourably. To-day there was a copious discharge of healthy pus. Two ligatures, which were attempted the day before, were removed without difficulty. The wound, which covers a space equal to the extended hand, was dressed with straps and a bandage. She was ordered nourishing diet.

NECROSIS OF THE TIBIA—AMPUTATION.

Archibald Macintyre, aged 46, was admitted by Dr. Couper, Feb. 11th. At the upper and anterior part of the right knee joint, there was a large, foul, ulcerated surface, which extended downwards to seven inches along the tibia. In the centre of this ulcerated surface, there was a wide orifice covered with thick tenacious matter, in which the tibia was seen black and necrosed. It was quite soft and friable, the probe passing easily into its substance. From this opening there was discharged a considerable quantity of thin ichorous fluid. The leg was of natural warmth, but the foot was cold and œdematous.

He states, that fourteen years before, he was struck on the upper part of the knee joint, which afterwards swelled. The inflammation was subdued, but confirmed anchylosis took place. It is at present bent, but gave him little uneasiness, till December last, when the joint and the upper part of the leg became affected with severe inflammation. The skin over the tibia shortly afterwards broke, which was followed by a copious discharge of thin fluid, and rapid extension of the ulceration. In the beginning of January, several pounds of blood issued from its surface. The patient was sallow and much emaciated, but his appetite continued good. He also slept well, but was troubled with profuse perspiration. His pulse was 140, and small. He had eight ounces of wine, and an anodyne draught.

13. Dr. Couper removed the limb to-day by the circular operation. Mr. Cowan applied the tourniquet at the lower part of the superior third of the thigh. The amputating knife was carried round the limb, dividing, as usual, the skin and integuments. These were dissected up for a considerable extent, with the same knife that had been used in making the circular incision. After this was accomplished, the muscles were divided to the bone. A scalpel was now handed the operator, with which he detached the muscles from the bone for an inch, or perhaps a little more. The retractor was now put on, and the bone sawn through. The femoral artery was immediately tied, but some time was occupied in applying ligatures on other three vessels, which, from the weak and emaciated state of the patient, had ceased to bleed, and were more difficult to secure. The integuments were now brought over the face of the stump, and the usual dressings applied. While doing this, some care was necessary to keep the surfaces of the wound accurately together, from retraction of the skin.

When the man was removed, the limb was dissected. It seemed a complete mass of disease. The tibia immediately beneath the joint was necrosed to a considerable extent. The joint was completely anchylosed, requiring considerable force to effect the slightest movement. The cartilages were almost entirely absorbed.

FISTULA IN ANO; STETHOSCOPIC IGNORANCE.

David M'Lardy, ætat. 21, dyer, was admitted on the 29th Dec., labouring under fistula in ano; four years since had an abscess in site of present opening, which was punctured and shortly after healed; about five months ago observed a discharge from anus, which has since continued; general health good: ordered half an ounce of castor oil immediately, which produced one stool. Fistula was laid open on the 4th Feb., after which he had griping and uneasiness in bowels; on 7th, had a rigour, since which has been feverish and uneasy, with severe pain in back and testicles; pulse 90; tongue whitish; thirst; urine high coloured; ordered to be bled to ʒxij, which presented a buffy coat. The pain in testicles and other feverish symptoms continuing, the hip-bath, with a pill of calomel and opium, was ordered to be given in the evening, which had the effect of allaying pain and producing profuse perspiration.

15. Had a rigour; pain in back and testicles; lower part of abdomen slightly tympanitic, but not painful on pressure; pulse 94; tongue cleaner; thirst: twelve leeches to abdomen, after which had an enema, followed by one stool.

14. Twelve leeches to left hip, which was painful on pressure.

16. Passed a good night, but since morning severe pain of back; pulse 100; bowels moved by enema; ordered to repeat the pill immediately, with the hip-bath and Dover's

powder in the evening. With the exception of some delirium and a slight abscess over left hip, which was punctured, no other symptoms occurred until the 25th, when he was attacked with frequent cough, accompanied by bloody expectoration, for which he was ordered a blister to breast, and cough mixture. Debility gradually increasing; died on the 29th.

Sectio cadaveris.

The abscesses over hip had contracted to a small size; the colon was found distended with air, and the abdominal viscera perfectly free from disease; the right lung adhered extensively to the walls of the thorax, and with the exception of a portion of its anterior part, was either totally hepatised or entering into a state of purulent infiltration. Mr. Cowan mentioned at the post-mortem examination, that his attention had been completely confined to the state of the abdominal viscera, never having suspected disease in the lungs, though declaring at the same time, that patients labouring under fistula in ano were very obnoxious to pulmonary complaints. If he were aware of the liability of such complaints, why was not the attention directed, at an earlier period, to the state of the lungs? Why not avail himself of the stethoscope, of the use of which, he *declared himself perfectly ignorant*? What apology has a surgeon, filling a public situation, for such ignorance, and that too, when it could be so easily remedied, in this instance, by transferring a portion of the servile obeisance paid to the practice of Lisfranc and Dupuytren, to that of Laennec?

COMPOUND FRACTURE; IMPROVED PLAN OF TREATMENT.

James Birney, ætat. 30, labourer, a man of intemperate habits, was admitted Jan. 7, with compound fracture of right leg, in which both bones are fractured; the tibia laid bare to extent of two inches; two detached portions were extracted, and about a quarter of an inch of its upper shaft sawed off, after which, the edges of wound were brought together and splints applied. The patient was in a higher degree of excitement than could have been produced by the quantity of wine he received previous to admission; general health good; ordered a pill of calomel and opium immediately, and a cathartic bolus in the morning; after which the patient continued in as good a condition as could be expected, until the 25th, when he had a rigour, in consequence of which, it was thought necessary to dress the leg, for first time. Tibia is still exposed, though grater part of wound is closed with healthy granulations; discharge moderate. After dressing, pulse had sunk from 100 to 86; ordered Dover's powder.

26. Diarrhœa and griping accompanied with tenderness of abdomen, on pressure, but, which was partially relieved by opium. Leg dressed, after which, it not being very accurately fixed, he was desired rather roughly to "turn his foot." (Who ever heard of a man with a fractured leg being desired to turn his foot? Oh! spirit of Mr. Pott, what would you exclaim on seeing such treatment?) Calomel and opium were ordered in large doses, with a blister to abdomen, which had no effect in relieving diarrhœa.

28. Leg dressed; discharge healthy; no bony union; which appeared to be prevented in a great degree by the leg being always raised from its pillow, during the dressing.

Feb. 2. Diarrhœa continues; complains of debility; countenance sharp and anxious; pulse 100; tongue dry; wound dressed; discharge healthy; upper shaft of bone protruding; medicine continued, with the addition of sulph. quinine, during the day.

6. Slight cough; no pain in chest; expectoration sinks in water; wound as before.

7. Patient left the house, having the foolhardiness to believe that "sound chirurgical" might be had elsewhere than within the walls of a hospital.

BURN.

Mary M'Cosh, ætat. 40, a woman of intemperate habits, was admitted on the 2d Feb., with severe burn, extending over the integuments of face, right side of neck, greater part of thorax, and considerable part of right arm and shoulder. The left hand and forearm, and anterior parts of left knee are also scorched and slightly vesicated.

4. Violent delirium during the night, with tremor; pupils contracted; tongue furred; thirst; bowels open; pulse 116; four ounces of alcohol during the day.

5. Constant delirium continued until the 6th, when she fell asleep.

9. Involuntary motions; sores dressed; slough undetached; slight purulent discharge; medicines continued. The symptoms remaining unabated and the debility increasing she expired on the 16th.

Sectio cadaveris.

Serous exhalation over the hemispheres of brain, with a collection of bloody serum in basis cranii; partial hepatisation of right lung, of long standing; two or three small abscesses under integuments of abdomen; liver in some parts cartilaginous.

It was mentioned, at the post-mortem examination, that the patient had been bled after her admission; the delirium tremens with which she was affected being deemed the effects of cerebral congestion. Query! Is it usual to bleed patients affected with severe and extensive burn? Why was not the *flour* applied?

THE LANCET.

London, Saturday, March 7, 1829.

A Medical Journal has recently been established at Paris, under the title of *La Lancette Française*; the Editors of which state, that in the conduct of their publication they will be governed by the same principles of impartiality and independence which characterise the English Journal whose designation they have adopted; and that one of their main objects is to present the Medical Profession in France with faithful reports of cases occurring in the public hospitals. Hitherto, reports of hospital cases have, for the most part, been transmitted to the Editors of French Medical Journals by the hospital physicians and surgeons themselves, and the consequence has been that no reliance could be placed on the fidelity of the reporters. It is not necessary to resort to the invidious supposition that the French hospital functionaries stand lower in point of honour and morality, than persons holding similar situations in other countries, in order to account for the system of garbling and misrepresentation, which, it seems, has long notoriously prevailed in the reports of hospital cases, as published in the French journals. It is a law of human nature, which most men discover and act upon without the help of Puffendorf, or Grotius, that wherever they have the uncontrolled power of praising or censuring themselves, they will generally give themselves the full benefit of their own approbation, and avoid giving unnecessary prominence to unfavourable particulars, or, in other words, suppress whatever may tend to injure their own interests and reputation. Hence the publication of hospital reports by hospital physicians and surgeons themselves, leads, of necessity, to unfairness in the selection of cases laid before the public, and, for the most part, to a partial representation of the facts of the cases selected. Even in the best conducted hospitals errors must be occasionally committed, but no man will voluntarily become the historian of his own blunders; and the amount of fraud practised on the profession and the public, by the suppression or misrepresentation, of cases, which *must* exist to *some* extent, even on the most favourable supposition, will be less or greater in proportion as the hospital functionaries are more or less competent to discharge their official duties. In this country, where the system under which hospital patronage is distributed affords no security for the competency of the persons appointed to discharge the duties of hospital surgeons and physicians, and where we find a single individual able, by dint of corrupt influence, to quarter his whole family upon two charitable institutions, to the exclusion of men of vigorous intellect and unpatronised talent, what would be the result if the profession and the public had no other records of hospital proceedings to look to, except such as were supplied by the parties who are interested in concealing their own inefficiency? We are not left to conjecture the consequence, for the experiment which has actually been made by some of the hospital surgeons, has enabled such members of the profession, as their lucubrations have reached, to estimate the degree of honesty and impartiality with which they have recorded their own professional exploits. It will be recollected that, at the late trial, we elicited from the mouth of Mr. Brodie, in the witness-box, an admission that he, and some other hospital surgeons, had subscribed to pay the expenses of a publication, which gave them an opportunity of becoming their own panegyrists, and of paying weekly homage to their own perfections. We can make no better comment upon this admission of Mr. Brodie, than one which is supplied to our hands, in a passage of Sir James Scarlett's speech—

a passage which, like many others in the learned gentleman's address to the jury, affords an instance of the curious infelicity with which his arguments tended to strengthen the case which he was instructed, and, peradventure, paid, to oppose.

"What, if in the law in every assize town, persons were employed to publish the fame of their own particular efforts, and not to allow that fair competition in a court of justice, to determine who is the advocate the public should employ, but to give it out beforehand, by suppression and false representation, would not that destroy the honour of the bar, and would it not tend still more to degrade that honourable profession, if among themselves persons were found capable of making reports of their own exhibitions? Such a thing cannot happen in the profession to which I belong, but it has an example in the medical profession."

We may observe here, that before any conclusions can be fairly drawn as to the degree of animadversion to which the persons styling themselves the *heads* of the medical profession ought to be subjected, from the manner in which the public are enabled to judge of the abilities of gentlemen practising at the bar, it must first be seen whether there is any thing parallel in the state of the two professions. What, if Sir James Scarlett were able to procure silk gowns for half a score of his nephews and immediate connexions, would not such a fact of itself raise a strong presumption, that there was something radically corrupt in the system under which the honours of the legal profession were distributed? Who ever heard of a family circle of King's counsel, or what power has even the highest law officer of the Crown to confer pre-eminent *legal* station upon his relatives and connexions? Sir James Scarlett could hardly have fallen upon a more forcible illustration of the difference between a profession in which the sole claim to distinction is superior ability, and a profession where official station is procured by family influence and corruption, than that which he unwittingly employed in behalf of his client, Mr. Bransby Cooper. In the profession of the law, the race can only be won by the strongest; in that of medicine, the weakest are smuggled up to the goal at the moment when those who rely upon their own exertions receive the signal for starting. We believe there is hardly an instance on record, in which a lawyer, who has distinguished himself in his profession, has seen a son or relative acquire honours in the same career; a proof of the condition upon which alone honours in that profession are to be obtained. But what is the case in the medical profession? In two of our principal charitable institutions for the relief of the sick, we find a single individual openly avowing, that the official situations are all held by himself and his immediate connexions. We find Sir Astley Cooper, and his nephew Tyrrell, and his nephew Key, and his nephew Bransby, and his godson Green, and his apprentice Morgan, and his apprentice Travers, all flourishing upon the same stalk at Guy's and St. Thomas's Hospitals! Under such a system as this, is it surprising that medical science should be at a lower ebb in this country, than in any other country in Europe? Under such a system as this, is it surprising that the degradation to which the medical profession has so long submitted in this country, should have influenced the estimate which men of science on the Continent are disposed to make of the character and pretensions of English medical practitioners? Of what materials, it is natural for the uninitiated to ask, must that profession be composed, of which nephew Tyrrell, and nephew Key, and nephew Bransby, are the *heads*? The system speaks for itself; and Sir James Scarlett, one would think, must have chuckled internally at the grossness of the sophistry by which a jury of merchants could be persuaded, that there was the smallest analogy between the means by which honours are obtained in the profession of the law, and those by which *newsy*

and noodles are smuggled into office in the medical profession. There are other reasons, perhaps, which may account for the good understanding which subsists among lawyers, besides that which is founded upon the result of free and open competition. Next to the *odium theologicum*, the wrangling among members of the medical profession has been immemorially noted for its acrimony; but lawyers have been likened to Swiss mercenaries, who get too much by the quarrels of other parties, to lose time and money by engaging in quarrels among themselves.

> The Galenist and Paracelsian
> Condemn the way each other deals in;
> Anatomists dissect and mangle,
> To cut themselves out work to wrangle;
> But lawyers are too wise a nation
> T'expose their trade to disputation.
> They have no interest in the cause,
> For which t'engage, and urge the laws,
> Nor further prospect than their *pay*,
> Whether they lose or win the day.

This, it must be admitted, is rather a vulgar, common-place view of a profession, the members of which, as Sir JAMES SCARLETT has assured us, are honourably distinguished from commercial "*hirelings*," who are sordid enough to receive money in exchange for their labour; it is, however, the view of a poet, and of a poet whose wit, and fancy, and unrivalled power of making human learning subservient to the illustration of human folly, will excite admiration, when even Sir JAMES SCARLETT, and his Poor Bill, shall be utterly forgotten.

Among the hospital surgeons there is one singularly-gifted individual, who, if we may judge from the display of intellectual power which he made when he delivered the celebrated Oysterian Oration, would derive but little benefit from the privilege of reporting his own cases. This eminent person has threatened, it seems, to withdraw his countenance from the profession, if its members should continue to manifest that horrid predilection for THE LANCET, which the enlightened orator indignantly denounces as one of the most awful signs of the times. Sir ANTHONY CARLISLE complains that he has not received that treatment at our hands to which his age, his knighthood, and his oratorical acquirements, fairly entitle him; and we are free to confess, that, *quoad* orator, we humbly conceive Sir ANTHONY to be the most unmitigated ass that ever disgraced the character, and exhausted the patience of the profession. Estimating his intellectual size by the Oysterian Oration, we know of nothing low enough in the animal or vegetable kingdoms which we should not disparage by likening to the capabilities of Sir ANTHONY: judging of him by the Oysterian Oration, we should have no hesitation in saying, that he stands confessed the Thersites, the jack-pudding, the wooden spoon, the boots, of the medical profession. This is our honest opinion of the *sayings* of Sir ANTHONY; but as to his *doings*, we will pledge ourselves that on the few occasions on which it has been necessary to notice them, (for let it be borne in mind that he visits but twice a week the hospital of which he is one of the appointed surgeons,) these have been faithfully recorded in the pages of this Journal. He has not ventured to point out any inaccuracy in our reports from the Westminster Hospital, and until he does so, we shall not notice the impotent twaddle which fell from him, on a late occasion, further than by observing, that *he* is the last person in the world who has a right to complain of the licentiousness of the press, seeing that he has had the indecency to propose that obstetric practice throughout the kingdom should be transferred to the wives and daughters of medical men; and the baseness, as far as in him lay, to sow the seeds of domestic misery, and to ruin the peace of families, by insinuating that accoucheurs were in the constant habit of committing the foulest and most atrocious violations of the confidence reposed in them.

With respect to an observation which is

reported to have been made by Mr. GUTHRIE, at the Westminster Hospital, we have but one word to add. That gentleman, in alluding to a case of strangulated hernia, in which he had the misfortune to burst, or cut, the patient's intestine, in endeavouring to return the gut into the abdomen, seems to think it essential to his reputation to state, that he expressed his fears, at the time of the operation, "that the *texture* of the gut could not withstand the force necessary to restore it into the abdomen." The substantial question is, whether so much violence ought to have been used, and not whether Mr. GUTHRIE correctly foretold the consequences of that violence. There are no prophets more dangerous than those who have the power of fulfilling their own vaticinations.

WESTMINSTER MEDICAL SOCIETY.

February 28, 1829.

Dr. A. T. THOMSON, in the Chair.

BUFFINESS OF BLOOD—NEW THEORY.

Dr. GREGORY introduced to the Society the subject which had been so often postponed. It was one which he considered worthy their attention, and related to the well-known appearance of buffiness in blood. Of all the important topics connected with medicine, there was not one, perhaps, on which the minds of medical men were less clear; it came daily under their notice, and yet no ideas could be more indistinct than those they entertained on the subject; their reasoning was loose, and their speculations were vague, and it certainly deserved a better consideration than had yet been given it. Hebberdine, in 1762, had gone so far as to ask whether buffy blood was a phenomenon of sufficient importance to excite the attention at all? in other words, he had asked, whether sizy blood ought to be considered the index of the state of the body? and to this day there were differences of opinion on the question, some answering it in the affirmative, and some in the negative. He would take a short review of the opinions which had from time to time existed on the subject, and which perpetually contradicted each other. The ancients had devoted but little attention to it; Sydenham was the first person who had given it any serious attention, and he, in speaking of pleurisy, had laid it down as a rule, that buffy blood was materially connected with disease. He first noticed the fact of one cup of blood showing buffiness and another not. Then a new school arose, in which every thing was buffy blood; and the disciples of this, so overloaded the doctrine with its importance, that at length it got into disrepute. More modern times had reduced the subject within more resonable limits, and called attention more to the pathology of buffy blood; still the doctrines were so various, that they perpetually contradicted each other. Simpson, in his *De re medica*, asserted that buffy blood was owing to the blood being stagnated in its motion. He noticed that when a ligature was put on a vein, and the patient was bled from that vein, the blood became buffy; from this fact he deduced that all buffy blood was the effect of stagnation. But there were other philosophers who denied this altogether, and said that buffiness was the result of a great rapidity in the circulation. Next there spread a notion that buffiness was owing to the blood having become too glutinous; and then Hewson rose up and said that it was because the blood was too limpid. This was now the more general opinion; but there were some who said, that buffiness was owing to too much fibrin, which a third party wholly contradicted; and thus the medical world had gone on from period to period, contradicting and opposing each other, and involving the question in obscurity. Here were six theories on the subject, but not one of them near the truth. In fact, the phenomenon was to be contemplated in a very different view to what had yet been taken of it, and it was his (Dr. Gregory's) opinion that we must examine it as connected with its circulation. People look at buffy blood when it has been drawn, and form their conjectures and theories upon it accordingly, considering that the buffiness results from the abstraction of the blood; but he contended that the only proper light in which to examine it was, what were its state and effects while circulating in the system. It was a remark of old authors, that the blood in the spring had a greater tendency to buffiness, than at any other period of the year. Now he did not believe this. His opinion was, that buffy blood could not circulate in the body without inevitably producing disease; but disease did not always exist at that period in which it was alleged that buffiness was most prevalent. He should now draw the attention of the Society to his views of the subject; the basis of these was, that buffy blood was the cause of disease. Buffy blood differed in its kind; it varied in intensity; sometimes it had very little intensity; at others it was highly intense. He

had seen it in all its degrees, and it was from personal experience that he had drawn his conclusions. His observations had been chiefly made at the Small-Pox Hospital. Now it was commonly said, that buffy blood was the result of inflammation. But if buffiness depended on an increased action of the system, how was it that the blood was not always buffy in small-pox. During the height of pustulation, and when the inflammatory action was greatest in that disorder, there was no buffiness; but there came a period afterwards, when it was most prominent, and that too when the inflammatory action had really subsided; and it was curious to see how decided a change in the blood occurred from that very moment. He did not pretend to say in what that change actually consisted. He could tell well enough when it was buffy, but he could not tell why it was so. One of the first things, however, which might be noticed as the result of this change, was the occurrence of purulent ophthalmia, of inflammation of a purulent character about the eyes; the moment the blood became buffy, that moment the eyes became affected; the total disorganisation of the eye first commenced, and then the brain became disturbed, the buffy blood began to circulate through the brain, and when it was most buffy, life broke down; the brain would not bear it. When the brain, after these attacks, was examined, there were no appearances to account for death, but those of the existence of buffy blood in it. Now the theory which he held on this subject had been forced upon his mind; so prominent were the facts connected with it, that he could only draw one general conclusion from them—that buffiness was the cause of disease and of death. Other facts he had noticed especially, were those of the state of the blood in rheumatism, and they all tended to establish the same principle. He was convinced that, in chronic rheumatism, the blood was as buffy as in acute rheumatism; and if the opinions he held were correct, this ought to be the case; and so it was. He had heard people talk of metastasis in acute rheumatism. Now he asserted that this never took place until the blood became buffy; but the moment it did so, metastasis occurred. The buffy blood penetrated the heart, and it immediately brought on pericarditis and rheumatism of the heart, then it was that inflammatory action commenced. He had been lately attending a case of acute rheumatism, in which the buffiness had been carried to the highest grade of intensity he had ever seen. Now buffy blood had been asserted to exist almost always in the venous system, and very seldom in the arteries; but this case (it occurred in Wardour Street) was an exception, and every cup which had been taken, was deeply buffy. Here the blood had gone to the arteries, and produced its effects, but not to the brain. We often talked of diminished action, and increased action, and spasm, in the system. For his (Dr. Gregory's) part, he could not admit any of them to account for disease. He knew of nothing which could account for the symptoms which appeared but the buffiness of the blood, and it was upon this that his views of disease rested. He should next say a few words on the subject of its cure, he was of opinion that there was wanted a medicine which would alter the buffiness of the blood, and if this could be obtained, a most important point would be gained. There was nothing for the cure of buffy blood—that is, for the cure of disease—but the means of totally eradicating the buffy blood from the system, or the buffiness from the blood. When the last drop of this could be drawn away, then the patient would be well, and only then. He believed that colchicum, nitre, and such other medicines, were the most complete for this purpose, in cases of rheumatism, that had yet been obtained, but they were not fully effectual. There was wanted some other medicine, and he should now be glad to hear the opinions of such members as were willing to discuss the subject.

A Member, whose name we could not learn, observed that Dr. Gregory had omitted to take into account one cause of buffiness in blood in drawing his conclusions, and that was, that buffiness was generally produced by increased heat of the body, whether disease was present or not; a man, who had run with sufficient violence to heat himself, would always exhibit buffy blood. He should like to have Dr. Gregory's remarks on this fact.

Mr. Bennett would not follow Dr. Gregory through all his arguments, but content himself with objecting to the new theory, that if we allowed ourselves to use our senses, instead of our imagination, we should find, that the attempt to discover any really important difference between buffy and healthy blood, was almost unavailable. There was, in fact, no physical difference between them. The only real distinction was a difference in the red colour of their globules. He thought there were far too many terms used in speaking of the blood. Coagulable blood had only lost the red coats from its globules; in other respects, the blood was just the same thing. How could buffy blood be the cause of disease, if it often existed in rude health, as it did in pregnant women. In cases of ligatures, the blood was buffy, and yet there was no disease. If Dr. Gregory was accustomed to read The Lancet, and he (Mr. Bennett) could not say Dr. Gregory was not, he would have found

some time since an account of some experiments by Mr. Vines upon animals, exhibiting buffy blood, very strongly opposed to his theory. He agreed with him, that if the globules of blood had not got their red coats, that the blood would not support life, and that if it was sent to the brain it would affect it, as in the case of black blood. This time twelve months, he had brought forward a paper on the subject of inflammation,—in which he had stated, that in the first stages of inflammation, mere serum was effused, and, finally, that the whole blood was effused; and these facts were decidedly opposed to the present views of Dr. Gregory.

Mr. THOMSON inquired of Mr. Bennett, whether he considered that blood lost its redness (the salt of iron) before bleeding or afterwards; he thought himself it lost it afterwards, and that this difference had no effect on the circulation.

Dr. GRANVILLE expressed himself deeply disappointed at the result of Dr. Gregory's communication. This was the third evening he (Dr. Granville) had come, urged by his great desire to hear what it was Dr. Gregory had to lay before them; night after night he had attended with the greatest impatience, but now he thought the Society might have been thankful to Dr. Gregory, if he had told them once more, that he was married, and could not come. The question, however, was now fairly before them. Dr. Gregory was a man of great talent and pre-eminent ability, but he had expected much more from him than Dr. Gregory had given them that night. He particularly considered that he was deficient in his remarks on the physical state of the blood. He had passed this over in the most superficial manner. He ought to have given the Society his opinions of what buffy blood consisted, and its effects in the circulation—the mode in which it produced disease. He should have shown what was the texture and condition of the blood, whether the blood was or not a living fluid. These were the really important questions to found a theory upon; and he considered, that Dr. Gregory's views being thus deficient, were not entitled to any attention. Dr. Granville having then disposed of one or two "episodes" in his speech, impeaching the opinions of Mr. Bennett, returned to Dr. Gregory, and disputed the "practical" portion of Dr. Gregory's opinions. It was true, that where buffy blood was present, there was, generally, disease; but he should hesitate before he yielded his credence to the theory, that it arose from the buffiness; and the more so, because his experience went to prove, that no such thing as buffy blood did circulate in the vessels of the body. The buffiness did not exist until after it was extracted. Had Dr. Gregory forgotten, that in bleeding, the buffiness was often got rid of in the third cup, and yet that it often reappeared. It often happened, also, that a first cup would be buffy, a second not, a third buffy, and a fourth not. How could he explain this? Yet that this was the fact, he would appeal to the merest tyro that ever wielded a lancet. Dr. Davy had witnessed, over and over again, on bleeding soldiers, that in seven or eight vessels some would be buffy, and others not at all so, although the diet, temperature, quietness, and every thing else, were alike. The opinions of Dr. Gregory as to its penetration of the heart and brain, and producing such knocking-down effects on the patient, were equally groundless. But, finally, if any proof were wanting that Dr. Gregory had not fully considered the subject, although he had taken all this latitude of time for it, it was to be found in the opinions he had advanced as to the cure. If "eradication" was the cure, and a third cup was free from it, the disease ought to cease; then why did the buffiness sometimes appear again? (Hear.) In short he (Dr. Granville) considered the communication of Dr. Gregory full of oversights and errors, and he trusted he would reconsider his views on the subject.

Dr. COPLAND made some remarks on the nature and size of the globules of blood, which he considered as very various. He thought the blood partook of the vitality of the whole system; that the appearance of the blood was an index to the state of the system. There was no circulation of buffy blood in the vessels. Inflammation often existed without buffy blood, and often with it. In cases of febrile action, we seldom found buffiness in the blood prominent, but as soon as inflammation attacked the serous and fibrous structures, then it became buffy. As to the cure, he thought that instead of attempting to abstract the buffy blood, moderate bleeding, with large doses of depressing medicine, by diminishing the excitement of the vascular system, would gain the proposed end more rapidly. Upon the whole he thought that Dr. Gregory had brought forward his theory, more to show his ingenuity than any thing else.

Dr. JOHNSTONE hoped that though six systems had fallen to the ground, the seventh would be erected more firmly. Dr. Gregory had said he could distinguish by the symptoms of a patient when the blood was buffy. If he could really do this, he had made a most important discovery. The blood was generally buffy in pregnant women; now he thought it fair to expect that whenever a lady was pregnant, Dr. Gregory could discover it by the blood, and this was a highly important thing. The real question, however, must be, is buffy blood the effect or cause of disease? Dr. Gregory

said it was the cause; Dr. Copland, that it was the effect. Now, Dr. Gregory had instanced small-pox in proof of his views, and had said that it was a pure and beautiful instance of inflammatory disease. But he (Dr. Johnstone) asserted that it was not. In fevers, there was great excitement of the head, lungs, &c. and yet the blood had no appearance of inflammation. The arguments, therefore, drawn from small-pox cases were not good ones. He altogether disputed his conclusions drawn from the inflammation of the eye, and he considered also that the buffiness of alternate cups of blood in chronic and acute rheumatism, was not accounted for by this seventh theory. Facts, too, were opposed to the total abstraction of buffy blood curing disease. Rheumatism was to be cured better without blood-letting than with. Where then went Dr. Gregory's theory. And as to the sudden penetration of the heart and brain by buffy blood, producing death or near it, he was astonished at the argument. The truth was, that the blood must circulate through these equally alike at all periods of disease, and there were not the least grounds for the new opinions on the subject.

Dr. Gregory stated, that he had introduced the subject merely to have the opinions of the Society; but he saw now that this was out of the question, for, according to the four gentlemen who had spoken, buffy blood actually did not circulate in the system at all. Now, if he knew any thing whatever of physic—if he knew one fact better than another—it was this, that the blood was different at different periods in the body, and that buffiness was one of its states. There were inflammatory conditions, in which no earthly power could obtain buffy blood; and there were other states, in which come it would, trickling down the arm, and nothing could prevent it. Dr. Gregory then recapitulated his opinions, and would not admit that they had been refuted.

A discussion arose as to the buffiness of blood in pregnant women, the facts connected with which were, in a long argument, brought, by the speakers, to bear against the theory of Dr. Gregory. It was considered that the buffiness never arose out of mere pregnancy, but from some specific derangement connected with it.

Dr. Stewart inquired if Dr. Gregory considered that buffy blood was, under any circumstances, a pathognomonic sign of inflammation; another member asked if its appearance ought to regulate the treatment of disease, and whether the pulse was not a better guide.

A Member again referred to the fact of violent exercise producing buffiness without disease; and stated, that when a man suddenly fell and injured himself, the blood, on being drawn, was not buffy.

Mr. Samwell said, that horses, on being bled after exercise, always gave buffy blood; that when they had been some time quiet, it disappeared.

After a second discussion as to the size of the red and white globules of blood,

Mr. North expressed it as his opinion, that Dr. Gregory's theory could not surmount the fact, that pregnant women, who were in perfect health, often yielded buffy blood.

Dr. Granville having taken an opportunity to state, that he had delivered, or superinteuded the delivery, of more than 26,000, women. (A general laugh.) Dr. Johnstone having stated, that if his memory did not fail him, he had seen an artery yield buffy blood—and the Chairman having asked Dr. Gregory if, in all inflammatory disease, there was not a disposition to form new parts; and having hinted that buffy blood was more or less, according to its intensity, connected with the formative process, which hint Dr. Gregory considered an ingenious one—these things having passed, the meeting separated, with a hope from the Chair, that the subject would not be allowed to sink into oblivion, without further discussion on another evening.

A petition to the House of Lords, on the subject of anatomy, laid on the table.

LONDON MEDICAL SOCIETY.

March 2, 1829.

Dr. Shearman in the Chair.

STATE OF THE SOCIETY.—TREATMENT OF SCARLATINA BY MERCURY.—EXTIRPATION OF MALIGNANT TUMOURS OF THE EYE.—THE SOCIETY'S ANNUAL DINNER.

At this Meeting it is usual to make a disclosure of the state of the Society, which is generally done by the President. On the present occasion, the Registrar officiated. The only statement worthy of notice, respected the funds. A year since, the balance in hand was 100*l.* 14*s.* 1*d.*; since that period 266*l.* 12*s.* 6*d.* have been received, making in the whole 367*l.* 6*s.* 7*d.* The expenses for the past year have been 399*l.* and a fraction, leaving, therefore, the Society in debt to the amount of 32*l.* A considerable sum had been paid to the Registrar for making a catalogue of the library, which was complete, and ready for delivery. 120*l.* had been laid out in the repairs of the house, which were the principal means of absorbing the cash. The Registrar had no doubt that the Society would soon wipe off its

amount of debt, if it continued as prosperous as it had hitherto been.

The Registrar read the minutes of the last meeting.

Mr. Ashwell and Mr. Shearly objected to the correctness of the minutes in some points, which were afterwards corrected accordingly.

Mr. Kingdon wished to know from Dr. Ramadge, if he recommended the use of mercury, invariably, in scarlatina. It appeared to him that enlargement of the salivary glands frequently accompanied scarlet fever, and this had raised a doubt in his mind, as to the propriety of the use of mercury.

Dr. Ramadge considered the affection of the salivary glands as simply sympathetic. Enlargment of those glands frequently took place also in dentition. It was in the early stages of scarlatina that he recommended mercury.

Mr. Kingdon was happy to hear this explanation, because he had known cases where the salivary glands had been enlarged, where they had gone into suppuration under the use of mercury, had been benefited by the black wash, and when the mercurial application had been laid aside, had got better.

Mr. Gosset *read* (rather unusual among the members of this Society) from a manuscript the particulars of two cases of what he considered malignant tumours of the eye, which he had extirpated. The first patient was forty-six years of age. After the eye had been extirpated, he had used a piece of sponge wrapped in linen, as a plug in the globe, by which he had suppressed the hæmorrhage, and which he considered much better than dossils of lint. As the parts healed, diseased surfaces presented themselves again, which were subsequently removed. A third time diseased parts showed themselves, to which *concentrated nitric acid* was applied, but the disease still went on until the patient sunk. On examination, it was found that the disease extended through the *ethmoid bone*, and was only separated from the brain by the *dura mater*. There was a calculus found in the gall-bladder, as large as a pullet's egg, which, with the parts that had been removed during life, were exhibited to the Society. The liver had the *melanoid* appearance.

The next case was that of a young lady who had accidentally received a blow on the cheek and eye; inflammation followed; it subsided; inflammation came on again, and afterwards presented every appearance of what Mr. Wardrop had called *fungus melanoides*. In extirpation, it was found that the retina was ossified, a circumstance by no means common. The girl had recovered, and became perfectly well.

Mr. Tyrrell had seen a great many cases of this description, and was much inclined to doubt the malignity of the tumour in the case last related. He was also extremely averse to operations of this kind. Taking the average of operations for malignant tumours of the eye, it would be found that by far the majority of cases proved unsuccessful. He strongly recommended the exhibition of mercury, having witnessed the best effects to result from it. Scarcely in any case, except where the disfigurement was very great, or where the patient's life was subject to much risk by hæmorrhage, would he recommend the removal of the morbid parts. Where the operation was performed, there was no necessity for a compress, either of sponge and linen, or any other thing. He recollected four cases, two in which Mr. Green, one Mr. Lawrence, and one he himself, had operated, where simple pressure with the finger for five or ten minutes suppressed the bleeding, nor did it ever return; besides, a plug was greatly calculated to produce irritation, and thereby increase the chance of failure in the operation.

Mr. Callaway inquired to what extent Mr. Tyrrell would recommend the use of mercury, supposing it did not seem to repress the disease, and supposing it affected the salivary glands?

Mr. Tyrrell answered, as long as the constitution of the patient would bear it; it might be for weeks, months, or even a year. He had known an instance where a patient had been kept spitting a pint and a half every day for sixteen weeks, without the constitution afterwards having proved to be at all injured by the mercury. If the disease was not suppressed at all, but seemed to proceed under the exhibition of mercury, then, after a reasonable time, he would lay it aside; but he should regard an operation in such a case as only speculative.

Mr. Callaway agreed with Mr. Gosset on the propriety of having removed the tumour in the case he had last related.

Mr. Ashwell had seen three cases of this kind; in one, the patient had been kept under the influence of mercury for six weeks, in another for two months, and in another for three months. The disease in all proceeded. After a short period, when the mercury was supposed to have been got out of the constitutions, operations were performed, and, in two of the cases, with complete success.

Dr. Ramadge was quite astonished to hear of its having been necessary to keep a patient spitting a pint and a half a-day for any length of time, or even for one day. He regarded carrying the use of mercury to such an extent as wholly uncalled for.

The President informed the Society,

that as their Dinner would take place on Monday next, the Society would, of course, not meet that evening, except in the Freemasons' Tavern, where he hoped to see a very powerful muster.

CIRCULATION OF THE BLOOD IN THE VEINS.

To the Editor of THE LANCET.

SIR,—In the report of the proceedings of the Westminster Medical Society, for the 7th of February, No. 285, Mr. Bennett offered some opinions relative to the circulation of the blood in the veins, and appeared to think them, either unnecessarily numerous and capacious, or that they performed some other function distinct from the mere passage of the blood. It appeared also that the generality of the members did not agree with Mr. Bennett's ideas on the subject; at the same time it does not appear that any satisfactory reasons were advanced in opposition to Mr. Bennett's opinions; indeed there appeared a deal of obscurity concerning the increased velocity of the blood through the veins, &c. If the following observations on this subject may be thought not unworthy of a corner in THE LANCET, I shall feel highly flattered by their insertion. It may be said that I have not offered any thing new in these observations; that is not my intention, but merely from what we already know of the circulation of the blood, and its mechanical contrivances, with the effects known to arise from a derangement of any of its modes of action, to deduce a sufficient reason against Mr. Bennett's opinion, and to show good grounds for believing that there does not exist a greater quantity of veins than is necessary to return the blood to the heart, and that they do not perform any function distinct and independent of it.

I am, Sir,
Your obedient servant,
Feb. 26, 1829. A.

It must be recollected that the artery is a highly active vessel, provided with muscular fibres, and consequently possessing great force in itself; the fluid in the artery has been forcibly expelled from the heart, onward to its destination, and this expulsion is kept up by the contractile power of the muscular fibres surrounding the artery. The vein, on the contrary, is an entirely passive reservoir, totally unprovided with muscular fibres, consequently the blood flows to the heart, partly from the impulsive power given to it by the artery, and partly from its equality of pressure; for by making an opening into a vein, and introducing a tube, the blood will flow in whatever direction you choose to direct the tube; hence the use of valves to prevent the blood from regurgitating. It must, therefore, be seen that there exists a great difference in the force and rapidity of the flow of blood between the artery and the vein. If there were generally but one vein to an artery, the vein would be unequal to the task of conveying back the blood as fast as the artery expelled it; the balance could not be kept up, the heart would have sent forth more blood than it could procure again from its veins; the equilibrium would be destroyed. But we see that from the facility afforded to the return of the blood, and the mechanical contrivance of its conduits, the heart is, in a manner, enabled to regulate its admission. Of what great importance this is in the animal economy, fully appears from the arrangement of the vessels of the head and spine; the blood is sent to the head by means of the vertebral and internal carotid arteries, which last are not permitted to enter the cranium in a direct line, but are obliged to take an exceedingly tortuous course, opposing some degree of resistance to the flow of blood to the brain. But what is provided for its exit? Large sinuses, which, communicating with the internal jugular veins, afford an easy and immediate passage to the blood. In fine, let the immediate danger be remembered that would arise from an obstruction to the reflux of the blood from the brain and spinal cord, and there can no longer appear a superfluity of veins for the mere passage of the blood.

ERGOT OF RYE IN LINGERING LABOUR.

To the Editor of THE LANCET.

SIR,—Being a reader of your invaluable Journal, and perusing your reports of the late discussion at the London Medical Society, on the effects of the secale cornutum, or ergot of rye, I beg leave to trouble you with a case in which I made use of the ergot. I was called to Mrs. Hitton, Chappel-street, of this place, who had been in labour for twenty-three hours. When I arrived I found a debilitated woman, with lingering or ineffectual pains; upon examining per vaginam, I found the liquor amnii discharged, the os tincæ dilated, and well lubricated, the presentation natural. I waited two hours, and the pains still being ineffectual, I determined to try the ergot. I put only one scruple into four ounces of water, boiling it down to two; of this I gave her one half; ten minutes having elapsed, and the pains still weak, I gave her

the remaining portion; strong pains came on, and in twenty-five minutes both child and placenta were expelled. Had it not been for the ergot, she would, judging from circumstances, have been hours longer. My opinion is, that the ergot ought not to be used if the os uteri be not dilated, at least to the size of half-a-crown, or if the liquor amnii be not discharged, or if there be any preternatural presentation, or if the pains be totally wanting.

Your obedient servant,
EDMUND TAYLOR, Surgeon,
Middleton, near Manchester.
Middleton, Feb. 18, 1829.

EXTRACTION OF TEETH.

To the Editor of THE LANCET.

SIR,—The only excuse I can find for the positive and illiberal remarks of your correspondent, Mr. J. De La Fons, respecting myself, is, that he only read a short paragraph or two of my paper on the Extraction of Teeth. The *large* forceps, as I particularly stated, are for the *very large* molar teeth *only*, the small one being *generally* used for teeth of *all* kinds. The large instrument is lengthened in the jaws, that is, in those parts extending from the joint to the short extremity, for the express purpose of making them more convenient to use, the loss of power being compensated by the additional thickness of it in the fore parts, which, however, adds but little to its bulk or length, this latter being precisely the same as the key; so that the "ludicrous extent" of it exists only in the imagination of the writer himself, who seems to have mistaken the word jaws for claws. It will be evident, upon a single perusal of his first paper, and a glance at the diagrams, that my opponent did not confine his remarks on the use of the key to the molar teeth; but now his arguments are disputed, and his conclusions (not facts) doubted, he has taken fresh ground, and wishes to make trial on them alone. There is no occasion, but I should have no objection to meet him there, not, certainly, as he somewhere sneeringly observes, in order to give publicity to defeat on either side, but for the laudable purpose of eliciting truth.

The following conclusions I believe to be founded on facts:—

1. That in 49 cases out of 50, in which "the key is generally used," teeth of *all* kinds can be safely and expeditiously extracted with the forceps.

2. That whether teeth are first loosened by the wrench of the key, or by the lateral or circular motion of the forceps, they must, in the after part of the operation, to be extracted without injury to the jaw or fangs, be lifted out in a direction perpendicular to their sockets.

3. That the forceps is the safer instrument of the two.

4. That fewer teeth are broken with it.

5. And, above all, that the application and use of it are *much less painful* than those of the key.

In *proof* of the last assertion, I have the concurrent and valuable testimony of many of my patients, who, having submitted to the application of both instruments, would not allow the key to be used afterwards, dreading, as they truly observed, "that horrible wrench."

Let any person carefully examine the boasted diagram, and place in a similar one, not a *conical*, but such a tooth as I have now before me, whose fangs are half an inch in length, and ⅜ of an inch in breadth from fang to fang, the diameter, at the neck, being no more than ¼ of an inch, when he will have ocular demonstration, that to be extracted skilfully, it must be raised perpendicularly out of the socket.

Talk of "scioliasts," "torturing," and "cavillers," indeed! Let Mr. De La Fons, before he attempts again to teach old practitioners, pay some deference to the opinions of those who are not so ignorant as he supposes them.

Relying, Sir, on your usual impartiality for the insertion of this, I beg to say, that I am an admirer of your public spirit, and

Your humble servant,
J. PROWSE.
Bristol, February 18, 1829.

WEBB-STREET SCHOOL OF ANATOMY.

To the Editor of THE LANCET.

SIR,—As you have inserted in THE LANCET, of the 14th inst., a communication from a pupil of the Webb-street School, falsely accusing Mr. Grainger and Mr. Pilcher of having refused gentlemen admittance to the museum which they have lately opened, professedly for the benefit and accommodation of their pupils, I hope you will, in accordance with your usual liberality and strict impartiality, allow an early insertion to a few observations, in refutation of a charge so totally unfounded. Immediately I perceived the accusation in question, I called upon Mr. Grainger, who assured me that it was altogether false, and that he had offered unlimited permission to all those gentlemen who would apply to him or to Mr. Pilcher. I can most positively subscribe to the accuracy of

this assertion, as I have not only been permitted to visit the museum myself, but have generally found several other students there whenever I have been there. It may at first sight appear strange, that any individual should presume to write so untrue a statement; but I think the general expression of your correspondent's letter, and a brief exposure of some few individuals who, for the sake of annoyance, have wished to go into the museum whilst Mr. Grainger is preparing his lectures, will convince you, and your numerous readers, of the badness of the feeling under which the letter was penned. To whom your correspondent intended to apply the epithets "high and mighty," it is difficult to conjecture; if he wished to assert a plain fact, why not write in a plain and intelligible language. This species of scribbling is highly characteristic of the pusillanimity of its author, and strikingly expressive of the baseness of the motive which it is intended to cloak. The persons alluded to have, of late, been frequently in the habit of abusing Mr. Appleton, in the most ungentlemanly manner, and even conducting themselves, whilst in the museum, in a way more becoming children than gentlemen. Should those pupils still continue to disgrace themselves by insulting Mr. Appleton, and injuring the preparations in the museum, I hope they will forbear calumniating the character of Mr. Grainger, whose conduct, both as a man and a teacher, is unimpeachable, and altogether undeserving the slander of so contemptible an assailant as your correspondent, "A Pupil." VERITAS.

SPERMATIC ANIMALCULA.

THE invention of the microscope is a subject of very great interest, and the immense advantages derived from its use, in the discovery and knowledge of minute structures, are so important, that we need not be astonished to find several countries wishing to attribute the glory of the discovery to themselves.

Fontana, a native of Naples, is said to have been the inventor, about 1618; but Borelli, in his Microscopical Observations, bestows the honour on Zachary Jansen, of Middleburg, in Zealand, who, he says, first made it known in 1590. But this statement of Borelli's is suspicious, as he was anxious to bestow the merit on that city, to the authorities of which he had dedicated his work; besides which, he gives to the same Jansen the honour of the invention of the telescope, which, however, Jean Baptiste à Porta gave an account of in the year 1589, in a work entitled "De la Magie Naturelle." Comiens mentions this circumstance in his work on the nature and presage of comets. Hook, of the Royal Society, enjoyed great celebrity for the manufacture of microscopes, during the 17th century. Of course, since that period, great improvements have been made in their power and construction.

Both Hartsoeker and Leuwenhoek, claim the merit of having first noticed animalcula in the seminal fluid, but the latter was the first who published any account of them, which will be found in the 142nd Number of the Philosophical Transactions, in a letter to Brouncker, the then president; it is dated Nov. 1677, and headed, "Observationes de Natis è Semine genitali Animalculis." I subjoin an extract from that letter:—

"Eandem materiam (semen virile) non ægroti alicujus, non diuturna conservatione corruptam vel post aliquot momenta fluidiorem sartam sed sani viri statim post ejectionem ne inter labentibus quidem sex arteriæ pulsibus sæpiuscule observavi tantamque in ea viventium animalculorum multitudinem vidi, ut interdum plura quam 1000, in magnitudine arenæ sese moverent. Non in toto semine sed in materia fluida crassiori adherente, ingentem illam animalculorum multitudinem observavi; in crassiori vero semine materis quasi sine motu jacebant."

Hartsoeker's account did not appear till the following year, when it was published in the sixth volume of the "Journal des Savans." He there says, that in urine, which he kept some days, there were engendered little animals, much smaller than those he noticed in gonorrhœal discharge, and which have the figure of small eels. He examined the semen of a cock, and describes the animalcula found therein as resembling, in their form, young tadpoles (grenouilles naissantes.) Needham bestows the merit of the discovery on Hartsoeker. "I shall take," writes Needham, "as little notice as may be, in this short summary, of the almost inevitable mistakes others have made, in this matter, before me, and the hasty consequences they have drawn from appearances that naturally surprise, by their novelty; such surprise is but too apt to captivate persons, even of the most serene thought, much more the young and inexperienced, such as Mr. Hartsoeker was, when he first discovered the spermatic animalcula."

How Needham could have made this statement is to me astonishing, for I see not a shadow to believe that Hartsoeker was the discoverer. Even before any account was published by him, Huguens gave a sketch of the animalcula he saw in grain, and in other matters, in the 28th Number of the Journal des Savans, while no mention is

made of any animalcula by Hartsoeker till the 30th Number of the same journal was published, where will be found an extract of a letter from him to the editor.

That Huguens knew of spermatic animalcula, when his letter was written, may be judged of by the following observation: "I am able, (says the editor of the journal,) to say, that these animalcula are engendered by corruption and fermentation; but there is another kind which must have another principle, I mean those discovered by the microscope in the semen of animals, which appear to be born with it, and which are in such great numbers, that it seems to be entirely composed of them."* In many parts of Hartsoeker's works, mention is made of Leuwenhoek, with whom he appears to be extremely vexed and annoyed, and speaks of his letters to the Royal Society as "bas et rampant," and containing, says he, useless and chimerical observations. In Hartsoeker's "Suite des Conjectures Physiques," he states, that Leuwenhoek's first communication to the Royal Society was published on the 25th April, 1679. This is not true, for the letter of which I made mention before was printed, as I have said, in the 142d Number of the Philosophical Transactions, which Number is for the months of December 1677, January and February 1678.

Animalcula are found in the semen of all animals, and almost at all times; they differ materially from the infusory animalcula, and, in fact, are confined to this animal secretion alone. Their dimensions are different in different animals; neither do they correspond with the size of the animal, as might be expected. For, in the space of a grain of sand, 50,000 may be counted in the semen of a cock; in the roe of a codfish, 10,000; whilst the roe of the ruff (a fish a thousand times smaller than the cod,) the animalcula are as large as the others. In oysters, muscles, and other shell-fish, animalcula have also been noticed, resembling those found in the semen of higher animals.

In man, their form is not always the same at different periods of life; for if we examine the fluid contained in the vesiculæ seminales of an infant, which is not yet proper to generation, we shall find animalcula, but not of the same figure as those in the adult. There are, however, little bodies, in which, Hartsoeker presumes, are placed and enveloped the spermatic animalcula, as insects are inclosed in their nympha. Again, in old men, the semen will be found to have lost these animals, and in some they are either all dead, or so dull, as to survive but for a very short period.

Seminal fluid resembles, at first, coagulated milk; when examined with the magnifier, the cause of the opacity is not discovered, but when it begins to dissolve, and assume the colour of soapy water, if looked at with a microscope of small power, the irregular parts seem to be in an indistinct slow motion. This is from the action of the animalcula, infinitely more minute, of a globular figure, with a sort of filament, or short appendage.

The little animals possess a double motion, the one oscillatory, from right to left, and from left to right, the other progressive. This latter motion is doubtless owing to the oscillation of the appendage. According to Spallanzani, in twenty-three minutes, the motions of oscillation and progression diminish; and in an hour and a half, but few of the animalcula possessed, in the experiment he made, any visible movement, and those that still retained the power, were confined to the oscillatory, the progressive having altogether ceased, from want of power in the tail, I suppose, to propel the body onwards. When all are at rest, by diluting the semen with water, you may more readily examine their figure. Each corpuscule is not properly globular, but elliptic, and the appendage is not only longer than it appears, but the breadth is not equal throughout.

The motions of the animalcula are instantly stopped, if rain or even distilled water be added to fresh semen. Saliva, however, does not seem to affect them. The temperature of the surrounding atmosphere has great influence on their existence, for Spallanzani noticed, that when the thermometer stood at 36°, the motions of the animalcula ceased in 45 minutes; when at 47°, an hour and a half; in two hours at 49°; two hours and a half at 51°; three hours at 54°; and when at 55°, after the lapse of three hours and a half. He constantly observed, that as the heat was greater, the duration of motion increased; so that in the middle of summer the corpuscula continued to move for seven and three-quarters, and even eight hours. If the humidity be lost, they immediately become motionless, nor can they be revived if saliva be added. Now Needham, and with him the illustrious Buffon, state that they, on examining fresh semen, observed long filaments, ramifying on every side; these opened and divided into moving globules, trailing after them something resembling tails, but so far from being such, that they caused an oscillatory motion, and were nothing more than the viscid seminal substance. Buffon, therefore, classes them with the infusory animalcula, and says they are either produced by an evolution of organical parts, or by real vegetation. That this is not

* Journal des Savans, No. 23.

the case, Spallanzani, and more recently Provost and Dumas, have, I think, proved. They found that animalcula existed in the semen previous to any alteration or decomposition by the influence of the air, and that they are active in this fluid, even while included in the organs of generation. Spallanzani took the semen from a ram, fresh and vigorous, so that a friend cut away the epididymes, which are commonly full of semen, and immediately it was presented to the microscope, when numbers of animalcula were observed, and all were vivacious. But a more conclusive experiment was made by Provost and Dumas.* After bleeding to death a rabbit, by opening the carotid artery, they examined first the testicle, where they found them in motion and considerable numbers, having the general character of the eel, with the head rounded, stretched out and flat. The motion of the animalcula which they remarked, was not caused by simple attraction and repulsion, but by the rapid flexions and alternations of the tail, which gave them the progressive movement. The same was remarked in the fluid contents of the epididymes and vasa deferentia. In the vesiculæ seminales, too, they were found, though mixed with other bodies, spheroidal and larger, often agglomerating, and of various diameters. In the prostate gland, they noticed globules analogous to those of milk, but no animalcula. It would be tedious to give you the peculiarities of the animalcula in different animals examined by these gentlemen; I shall, therefore, conclude this, and return to it at a future opportunity.

JOHN P—R.

HOPITAL DE LA CHARITE.

EXTENSIVE LOSS OF SUBSTANCE, IN CONSEQUENCE OF GANGRENE OF THE LEFT CHEEK, AND OPERATION FOR RESTORING IT.

STEPHANIE JOLY, ætat. 22, was, on the 11th of April, 1826, admitted under the care of M. Roux, on account of the loss of the left cheek, so that there was a large opening into the cavity of the mouth, bounded anteriorly by the median line of the face, posteriorly by a line drawn perpendicularly from the external angle of the orbit to the lower jaw, below by the under lip, and above by a transverse line about half an inch below the inferior margin of the orbit, and having an irregularly quadrilateral form, and exposing the left nasal cavity, left side of the tongue, and the interior of the left maxillary sinus. The alveolar process of the upper jaw and the upper lip were completely destroyed, and only one incisor was left at the internal, and a molar tooth at the external extremity of the opening. The patient had two years previously been affected with typhus fever, which, having terminated in sphacelus of the left cheek, had left this frightful deformity, for relief of which, the patient applied at the hospital. On the side of the orbit and nose, the soft parts were rigid and hardly susceptible of any extension; but the under lip and the external portion of the cheek being very moveable and well nourished, seemed to afford a sufficient quantity of substance to fill up the opening, from the extent of which it was, however, anticipated, that this object would not be obtained but by several successive operations. The first operation was performed on the 25th of April in the following manner:—The internal margin of the upper lip, in the median line of the face, having been pared off, a perpendicular incision, about half an inch in length, was made into the under lip, and its external margin brought into contact with the rest of the upper lip, where it was kept by means of three sutures; the left portion of the upper lip was thus replaced by the external part of the under lip, and the lower angle of the wound of the latter, formed the left angle of the mouth. On the fourth day after the operation, the needles having been removed, perfect re-union of the parts was found to have taken place, and the preternatural opening was thus closed inferiorly by the artificial upper lip.

In the second operation, in order to bring the artificial upper lip in contact with the superior margin of the aperture, it was necessary to dissect off the integuments from the remaining portion of the maxillary bone; this having been done, and the margins of the parts in question being pared off, they were brought into contact, and kept united by means of needles. On the following day, violent fever and headach, with erysipelatous swelling of the wounds, ensued, so that it was necessary to withdraw the needles; the united parts immediately separated, and, after the cicatrisation, were in the same condition as before the operation. A third operation, similar to the last, was accordingly performed, but with no better success, so that M. Roux resolved to follow another plan; he divided the artificial upper lip from its union with the right upper lip, and the superior margins of the preternatural opening having been pared off, he brought them into close contact, so that the mucous membrane of the cavity of the mouth formed part of the external sur-

* Mémoires de la Société de Physique et d'Hist. Natur. de Genève.

face of the cheek. Needles were employed to keep the parts in contact, but when they were withdrawn, it was found that the operation had completely failed, and that no adhesion had taken place. The unsuccessful issue of the three last operations induced M. Roux to adopt a different method; he formed a flap from the ball of the left thumb, and applied it to the preternatural aperture, in the same manner as in the rhinoplastic operation, when the flap is taken from the arm; union seemed to proceed very favourably, when, on the fourth day, the patient having during the preceding night been agitated by violent dreams, the parts were found separated, and all hopes of their union were destroyed. Under these circumstances, M. Roux considered it advisable to make no further attempt at remedying the deformity, and to discharge the patient. In the spring of 1827, she applied, however, again, and as her health had evidently improved, and the soft parts of the cheek, especially the artificial upper lip, appeared to have increased in flexibility and size; the third operation, viz., that of dividing the new lip from its adhesion with that of the other side, and of applying it to the upper and internal margin of the preternatural opening, was repeated. The parts were kept in contact by five sutures; and when these were removed on the fourth day, perfect union had taken place.

Considerable inconvenience was produced for some days after the operation, from the lower eyelid being forcibly drawn downwards; the parts however soon extending, it regained its natural position; and when the newly united parts had completely cicatrised, all that remained uncovered was a triangular space between the perpendicular margin of the right upper lip, the septum nasi, and the portion of the artificial upper lip, which, in the latter operation, had been turned upwards.

A month after the last operation, this triangular space was also covered, by bringing the right portion of the upper lip in contact with the perpendicular margin of the left, the edges having previously been pared off. The sutures being removed, within a few days the parts were found united; and when complete cicatrisation had ensued, the left cheek was perfectly covered with skin; the deformity was, of course, still very great; the opening of the mouth being small, the left upper lip much depressed, and the under lip forcibly drawn upwards and to the left side, and protruding in its middle portion; all these parts, however, gradually increased in size, and the patient, whose courage deserves, indeed, no less admiration than the skill of the operator, left the hospital apparently with the greatest satisfaction, on the improvement in her condition.—*Gazette de Santé.*

HOTEL DIEU.

SINGULAR AFFECTION OF THE SPEECH FROM APOPLEXY.

Jul. Meunier, 25 years of age, having, from his fifteenth year, been subject to periodical attacks of headach, which regularly returned every month, was, on the 5th of March, 1828, in consequence of a strong emotion of the mind, seized with an apoplectic fit, followed by incomplete paralysis of the right side. Being carried to the Hôtel Dieu, he was immediately bled, and the next day was in the following state; the power of motion in the right arm and leg was almost completely restored, their sensibility, and that of the right side of the trunk, was excited in such a manner, as to produce, on the least touch, the most violent convulsive movements, and almost intolerable pain. The patient complained of a violent pain across the forehead; sight, smell, and taste, were unimpaired. The right angle of the mouth was somewhat depressed; the motions of the tongue were perfectly free, and he spoke very fluently, but his pronunciation exhibited a very curious deficiency; for although he was perfectly sensible of what he intended to say, he could not easily express his ideas, and constantly confounded, or altered them, in a peculiar manner. Thus, instead of clef, he said *clique*; of douloureux, *difficile*; of rêve, *vers*; of eau, *rau*; of soupe, *soulium*; of bouillon, *boujon*, &c. It clearly appears from these specimens, that he was not at a loss at finding the words, but was only incapable of forming some peculiar combinations of letters. He was, at the same time, perfectly conscious of the mistakes he made; and when speaking very slowly, often succeeded in avoiding them. The patient was repeatedly bled, and gradually recovered, so that within three weeks he was able to leave the hospital.—*Journ. Hebdomad.*

ST. THOMAS'S HOSPITAL.

STRANGULATED FEMORAL HERNIA.

M. H., about 50 years of age, was admitted into Queen's Ward, under the care of Mr. Tyrrell, in the afternoon of Tuesday, February 10th, with a strangulated femoral hernia of the left side. The patient stated that she had been the subject of hernia for about fourteen months previously, but had not before experienced any great inconvenience from it; the tumour had descended 36 hours before admission and was about the

size of a small egg, accompanied with great tenderness of the abdomen, &c. Twelve ounces of blood were extracted from the arm, the warm bath resorted to, and the taxis applied repeatedly without effect: the symptoms being urgent an operation was deemed expedient and accordingly performed by Mr. Tyrrell, between 11 and 12 at night. On opening the sac it was discovered to contain a fold of dark-coloured small intestine, and the stricture found to be very firm. After the patient had been put to bed, the parts were fomented, and the extremities being cold, warm water was applied to the feet. Pulse 100, intermitting at every tenth beat; she slept soundly for two or three hours during the following morning.

11. Ordered to take of
Calomel, 2 grains;
Opium, one-third of a grain, every three hours;
Common enema, with six drachms of *Castor Oil*, and to be repeated in the evening, if required; the first returned immediately, and the second soon after its injection, without any fæces; continue the fomentation.

12. *Infusion of Senna*, half a pint;
Sulphate of Magnesia, half an ounce to be administered as an enema;
Had four stools during the night.

13. Profuse diarrhœa; stools bloody and offensive, but abated towards the evening.
Calomel, 2 grains;
Opium, half a grain, night and morning;
Tincture of Opium, 30 minims immediately.
Brandy, 2 ounces;
Sago, and *Sirup*.

14. Diarrhœa returned. Pulse 104, sharp; complains of pain in the abdomen;
Infusion of Mint, with *Tincture of Opium*; 6 minims every 4 hours.
Wound adherent at the lower part but not above.

15. Vomited this morning, but less purged; some pain on pressure over the abdomen;
A cataplasm to be applied to the wound; 6 ounces of red wine daily.

16. Abdomen more tender; pulse 96, small and quick; tongue brown, dry, and furred; bowels have been moved once. Apparently not so well; very drowsy, and does not complain of pain unless questioned, but is perfectly sensible when roused. Take the mixture every eight hours.

17. Much the same; substitute gin for the wine.

18. Bowels open; no vomiting.

19. The wound was dressed to-day, and presented rather a sloughy appearance; pulse 120, weak; complains of no pain or sickness; occasional hiccough.

20. Evidently sinking; pulse 112, weak and intermitting; complains of pain after taking nourishment at the superior region of the abdomen; tongue less furred.

21. Pulse intermittent, but less feeble; wound sloughy; worn in the afternoon with vomiting.

22. Diarrhœa; spirits low; eyes dull, and suffused with tears; countenance pale and anxious.

23. Passed her stools involuntarily; extremities cold.

24. Pulse at the wrist almost imperceptible. Gradually sunk till six o'clock the following morning, when she expired.

On examination of the body thirty-one hours after death, the wound was sloughy and widely open; the fold of intestine which had been returned was of a very dark colour; there was not evidence of much inflammation of the peritoneum having existed, but it was soft, and easily lacerable, as was the whole of the intestinal canal, especially the small intestines, the mucous membrane of which was found to be ulcerated.

GUY'S HOSPITAL.

Accidents admitted at Guy's Hospital during the week, under the care of Mr. Morgan.

Accident Ward—Fractured femur.
Injury to ancle.
Injury to hip.
Dislocation of the humerus.
Ditto ditto.
Asphyxia.
Fractured clavicle.
Chapel Ward—Contused knee.
Fractured femur.
Injury to hip.
Fractured neck of the thigh bone.
Contused wound of the head.
Charity Ward—Hernia.

BOOKS RECEIVED FOR REVIEW.

Elements of Pathology and Practice of Physic. By John Mackintosh, M. D. vol. i. octavo, pp. 484. Edinburgh, Carfrae and Son. 1828.

A Treatise on Obstructed and Inflamed Hernia, and on Mechanical Obstructions of the Bowels internally; and also an Appendix, containing a brief statement of the cause of Difference in Size in the Male and Female Bladder. By Henry Stephens. In boards, octavo, pp. 191. London, Cox. 1829.

TO CORRESPONDENTS.

The letter of "A Bartholomew's Pupil" next week.

ERRATUM:—Page 638, line 18, 1st col., for three *read* fifteen.